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GENERAL BOARD OF HEALTH.

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SECOND REPORT

ON

QUARANTINE.

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YELLOW FEVER.

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WITH APPENDICES.

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*Presented to both Houses of Parliament by Command of Her Majesty.*

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LONDON:

PRINTED BY W. CLOWES & SONS, STAMFORD STREET,  
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SECOND REPORT

ON

Q U A R A N T I N E.

---

YELLOW FEVER.

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TO THE QUEEN'S MOST EXCELLENT MAJESTY.

MAY IT PLEASE YOUR MAJESTY,

ALTHOUGH the circumstances which first called our attention to the operation of the present system of Quarantine, led us to restrict our Report chiefly to the statement of evidence proving the inefficacy of Quarantine in averting the outbreak and checking the progress of Epidemic Cholera, yet we had received a considerable body of evidence showing the failure of Quarantine as a means of prevention in other pestilential diseases. Since that time we have received from foreign consuls through Your Majesty's Principal Secretary of State for Foreign Affairs, and from eminent medical officers and others, who have enjoyed peculiar opportunities of observation, a large amount of additional evidence; and as this further evidence appears to us to display the conditions on which the spread of epidemic diseases in general depend, as well as the impolicy of the Quarantine laws which are still in force in this and other countries, we deem it our duty, humbly to present to Your Majesty, a summary of the results of our more recent inquiries, which we now do in this our Second Report as follows:—

We represented in our First Report, that Quarantine is based on two assumptions; first, that epidemic diseases depend on a specific contagion, and secondly, that by the prevention of communication with infected

persons and articles, it is practicable to prevent the introduction of epidemic diseases into uninfected communities.

We apprehend the question of contagion has no necessary connexion with that of the importation of epidemic disease. The alleged importation of epidemic disease is a question of fact to be determined like any other fact by evidence. The evidence about to be adduced appears to us to be decisive against the allegation of importation, not because it is decisive against the hypothesis of contagion, but because it shows the utter insufficiency of the grounds laid to establish the fact of importation. In like manner with respect to Quarantine, if it were admitted that one true and invariable character of epidemic diseases is, that they are contagious, it would not follow that Quarantine can prevent or even check their spread. Whether Quarantine is capable of exercising any such influence or not, is a question to be determined like other questions of fact by observation and experience; and as we endeavoured to show in our former Report, the real practical question for the public and the legislature, is not whether epidemic diseases are contagious, but whether Quarantine can prevent their introduction and extension.

With respect to Epidemic Cholera, we have shown in our First Report by a body of evidence which has not been impugned, and which is generally admitted to be conclusive, that whether this disease be contagious or not, Quarantine has had no influence whatever in checking its progress, and that wherever, in the recent course of this pestilence throughout Europe, Quarantine was put in force as a measure of prevention, it was speedily abandoned as useless and even mischievous.

On the first irruption of this pestilence into Europe in 1831-2, every nation successively menaced by it, endeavoured to bar it from passing its frontier, by rigorous Quarantine, and by military cordons, but in every instance without avail. Again the like attempt was made in 1847-8, and again it was everywhere admitted to be utterly ineffectual. Though many medical men in Great Britain had long ceased to place confidence in these expedients, yet the constituted medical authorities appeared still to

regard them in some degree as securities; but founded on recent experience, the Royal College of Physicians of London have changed their former belief, have declared an opinion in accordance with that previously expressed by the General Board of Health, and have recorded their conclusion in the following words:—

“Cholera appears to have been very rarely communicated by personal intercourse, and all attempts to stay its progress by Cordons or Quarantine have failed. From these circumstances, the Committee, without expressing any positive opinion with respect to its contagious or non-contagious nature, agree in drawing this practical conclusion, that in a district where Cholera prevails, no appreciable increase of danger is incurred by ministering to persons affected with it, and no safety afforded to the community by the isolation of the sick.”

From the atmospheric or climatic changes which have in every country generally preceded an outbreak of Epidemic Cholera,—from the extent of its range traversing the globe in zones,—from the steadiness and in many instances the post-like regularity of its course,—from the number and distance of the points of its simultaneous outbreaks,—and from the wide spread infection of populations with symptoms indicating its actual presence and operation many days, and in some instances weeks, before it assumed its proper and developed shape,—it appeared in the highest degree improbable that a Quarantine vessel placed at the entrance of a seaport, or a line of soldiers guarding the frontier of a country, should exert any real influence in staying the progress of this pestilence; and recent experience has rendered the fallacy of this expectation so palpable, that the attempt to avert the visitation of Epidemic Cholera by such an agency has been not unaptly compared, to that of the countryman “who endeavoured “to pound out the crows, by shutting the park gates.”

Were there, then, no other epidemic diseases against which Quarantine is supposed to be a protection, we might justly consider that recent experience has established the fact of its uselessness: but as it appears to be still the opinion of some persons that the public safety requires the intervention of Quarantine, particularly against Yellow Fever and Plague, we now proceed to state such information as has been submitted to us, respecting the

practical value of Quarantine as a security against the first of these two diseases.

With respect to YELLOW FEVER, medical authorities are divided as to whether this disease is essentially different from the ordinary fever of the countries in which it is prevalent, or whether it is the same disease differing only in intensity; but the weight of authority appears to be in favour of the latter opinion. The College of Physicians of London have recently pronounced a decided opinion in accordance with this view, in the following terms:—

“ After a very careful consideration of all the facts and arguments adduced on both sides, the College are of opinion that sufficient grounds have not been laid for stating that Yellow Fever is a disease *sui generis*.”

From the descriptions of the most careful and experienced observers, it appears that fever, within the Yellow Fever zone, prevails under three forms more or less distinct—the Intermittent, the Remittent, and the Continued. The Intermittent, of course, prevails chiefly in marshy and swampy districts; the Remittent, which is the ordinary endemic, or acclimating fever of the country, also prevails chiefly, but by no means exclusively, in similar localities; the Continued, in like manner, sometimes prevails in marshy and swampy districts, and then seems to be based on ague; but at other times it prevails where there is no marshy ground, presents no manifest character of ague, and approximates to the Continued type of colder climates. It is to this more continued form of the disease, whether prevailing on marshy ground, or otherwise, that the name of Yellow Fever is commonly given. But the highest authorities are agreed that these varieties do not constitute distinct and essentially different diseases.

We observe, indeed, that the medical men, who have had opportunities of personally witnessing this disease on the largest scale, and under the greatest variety of circumstances, have, with scarcely an exception, arrived at the conclusion, that Intermittent, Remittent, and Yellow Fever are modifications of the same disease influenced by

peculiar conditions. Were it, however, universally admitted, that Yellow Fever is, on the contrary, a disease *sui generis*, this would afford no reason whatever for its being on that account peculiarly amenable to quarantine.

Though fortunately the inhabitants of Great Britain are not within the range of a Yellow Fever epidemic, yet the subject of Yellow Fever is one which deeply concerns the British people. This pestilence is the great and constant scourge of the British army and navy, within the yellow fever region and near its limits; though its ravages in the army are probably less upon the whole than among colonists in general. From the statistical reports of the British army for the twenty years, from 1817 to 1836, it appears that in Jamaica with an average annual strength of 2,578 men, the average number of deaths annually from all causes was 313, and from all fevers 263; and of this number the deaths returned under the designation of Intermittent, Remittent, and Yellow Fever amounted to 258; that is, during a period of twenty years, all the troops in Jamaica that died of fever, with the exception of an annual average of five cases, died under one or other of these forms of this disease. So in the Windward and Leeward Islands, with an average annual strength of 4,333 men, the average deaths annually from all causes being 340, and from all fevers 160, the deaths from one or other of the above-mentioned forms of fever were 123. But in the Gambia during 19 months, the only period in which white troops were stationed there, out of a strength of 420 men, there perished from all causes 279; and out of this number 234, or about nine-tenths, were destroyed by this disease. Taking the experience of twenty years it appears that the average annual mortality from Intermittent, Remittent, and Yellow Fever alone in the West Indies and Western Africa, among a body of men at the age of least mortality, and selected with especial reference to their physical strength (each recruit having to undergo strict medical examination before being admitted as eligible for the service), is more than three times as great as the average annual mortality from *all causes* among the mixed population of sick and healthy, old and

young, in our own country ; while at the Gambia during 19 months, the mortality from this disease appears on a similar comparison to have been seventeen times as great.

The opinion entertained of the origin, mode of propagation and localizing conditions of this pestilence, must necessarily influence the measures recommended for the security of the men exposed to this extreme danger. The facts we are about to present have, therefore, a highly important bearing on the means of protecting troops in all British colonies subject to the visitation of Yellow Fever, as well as on the general question of Quarantine.

Within the last forty years Yellow Fever has been the subject of careful and extended observation. Medical practitioners resident within the limits of the Yellow Fever region, medical officers of the British army and navy who have witnessed formidable outbreaks of the epidemic in the places in which they have been stationed, and foreign physicians, some of whom have made it the great object of their professional life to acquire a knowledge of this pestilence by studying it in the countries in which it is prevalent, have given full accounts of the results of their investigations. Among these inquirers the first place must be assigned to a French physician, Dr. Chervin, who, with a devotion rarely equalled, passed eight years in the countries ravaged by this pestilence, with the sole view of studying it in its true and proper seats. In the prosecution of this object he travelled through all the colonies of France, England, Spain, Holland, Denmark and Sweden, together with every part of North America in which the disease is known, having in this pursuit traversed over and made investigations in 37 degrees of latitude.

Another distinguished investigator, a countryman of our own, Dr. William Fergusson, who resided upwards of twenty years in the Yellow Fever region, and as principal medical officer in the Leeward Islands had the most extensive opportunities of observing the true character of this disease, has left a highly valuable record of the results of his long official experience.

Two of our medical inspectors, Dr. Milroy and Dr. Gavin, during their late mission to the West Indies to aid in arresting the progress of epidemic cholera, have

collected, from their own personal inquiries, valuable information respecting Yellow Fever. Dr. Gavin has recently been an eye-witness of a severe outbreak of the disease in its epidemic form.

We are indebted to Dr. Gillkrest, Inspector-General of Army Hospitals, for a valuable monograph on one of the most carefully observed outbreaks of Yellow Fever on record, namely, the Yellow Fever epidemic at Gibraltar, in 1828. An abstract of this important paper recently presented to the Academy of Medicine of France, has called forth the high approbation of that body, and we have given the original document at length (Appendix No. I.,) as being not only an account of this particular epidemic by an eye-witness, in which the main events are ably analysed and succinctly described, but as recording the general experience of a long professional life devoted to the study and history of this particular disease.

We also subjoin (Appendix No. II.) the Official reply of T. Jones Howell, Esq. Judge Advocate, and Judge of the Vice-Admiralty Court of Gibraltar, to Secretary Sir George Murray, respecting the origin of the Yellow Fever epidemic of 1828, in that garrison; this paper being a close and judicial examination of the nature and value of the evidence presented to the Commission.

We further append "Remarks on West Indian Fever," by Dr. A. Browne, (Appendix No. III.,) presenting a summary of facts and observations with reference to fever in the West Indies, and coast of Africa, "contained in the statistical Reports for the Army from 1817 to 1836, and for the Navy from 1837 to 1843."

And lastly, we subjoin (Appendix No. IV.) a Notice of the proceedings of a Board of Inquiry held at the office of the Army Medical Department (1849-50), on the subject of Yellow Fever, with a Report by Dr. Burrell, presented to the Lords of the Council, and containing the reasons for the opinions delivered by him on that occasion.

In treating this subject in its practical rather than in its scientific aspect, it is necessary to advert to certain similarities, as well as to certain differences, which distinguish Yellow Fever, as an epidemic, from Cholera, which was the special subject of our former Report.

From the information derived from the sources above mentioned, as well as from others, it appears that Yellow Fever differs from Cholera as an epidemic in two important circumstances: first, in being limited to a certain zone, and secondly, in attacking chiefly those who have recently arrived within that zone, or near its limits. While Cholera spreads from continent to continent, ravaging the nations of all climates, unchecked by elevation of temperature, and often prevailing in intense cold—the seats of Yellow Fever are strictly confined to those parts of the tropical or equatorial regions, in which there prevails for several weeks in succession a steadily continued but not extremely elevated temperature; that is, where the thermometer ranges from  $76^{\circ}$  to  $86^{\circ}$  of Fahrenheit, and varies not more than from five to ten degrees night and day. Hence the West Indies, portions of North and South America, the coast of Africa, and the south of Spain are its principal seats, and it is stated to have appeared at Lisbon (1728), and in Leghorn (1804). Cases of it are said to have been observed in India on more than one occasion, but there is no record of its ever having prevailed there as an epidemic; it is generally conceived that *extreme* heat and drought are unfavourable to its development; and it appears to be incapable of existing in a cold country, or even in the very heart of its own region during the prevalence of a cold wind.

“Yellow Fever,” says Dr. Drake, “which has repeatedly prevailed in almost every town up the Mississippi to Vicksburg, N.L.  $32^{\circ} 24''$ , has never but once reached Memphis in  $35^{\circ}$ , and has not prevailed at any intervening town. Thus its limits, on the whole, have been those of the live oak, cypress, and long moss; and it will not, any more than they, be found among apple orchards, wheat fields, and groves of blue ash, sugar maple, and the arborescent buckeyes.”

The other characteristic distinction between Epidemic Cholera and Yellow Fever is, that while Cholera acknowledges no acclimatization, attacking equally natives and new comers; the acclimated creoles, natives of the Yellow Fever zone, escape the disease nearly altogether. The whole history of Yellow Fever shows that its most suscep-

tible subjects are those who have recently arrived within its sphere, particularly the inhabitants of northern climates, and that the predisposition to an attack, increases with the degree of the northern latitude from which the stranger has arrived, and the shortness of the interval that has passed since he left the European for the Equatorial region.

In illustration of the comparative security of native inhabitants over new comers, Dr. Fergusson adverts to the experience of the troops at Cape St. Nicholas Mole, St. Domingo, among whom soon after disembarking, Yellow Fever broke out "at every station and in every place." At the commencement of the epidemic a census was taken of the inhabitants of the town exclusive of the negro slaves; they were found to be very nearly equal in numbers with the newly arrived white soldiers. At the conclusion of the epidemic when 1,500 soldiers, *the original complement of the men*, had perished, the inhabitants had lost not more than one in thirty of all ages.

Dr. Burrell states that:—

"Of thirty regiments that arrived in the Windward and Leeward Islands between 1816 and 1848, ten were attacked with black vomit fever a very short time after landing; two within three months; eleven within twelve months; five within two years; and two within three years of their arrival. Of thirteen regiments which landed in Jamaica between the years 1816 and 1834, four were attacked within six months; seven within twelve months; and two within eighteen months. From 1838 to 1848, seven regiments arrived in that island, but the emancipation of the negroes permitting the troops to be quartered in the mountains, a few cases only of black vomit fever appeared, within that period, in two of them soon after landing."

The practical conclusion drawn by the most eminent medical authorities conversant with this disease, from these its peculiar characteristics is, that it is as impossible for Quarantine to afford protection against Yellow Fever, as it is for it to afford protection against Cholera. The fact and the inference are thus stated by Dr. Fergusson.

"A body of English troops arrives in a West India colony, and soon after the Yellow Fever breaks out amongst them. The seasoned Creolised white inhabitants feel little or nothing

of it, and the coloured classes without exception, the most numerous by at least 10 to 1 of the inhabitants, with whom all strangers are ever in necessary communication, stand by absolutely untouched. How is this? Import Small-pox, or any truly contagious disease, and they will suffer far beyond the usual sufferings of Europeans. Take them to England, they will be as liable as ourselves to fall under the dominion of Typhus Fever; to the Levant, under that of the Plague. Of Psora (itch) and Syphilis and every other infection, they will have their full shares. Yellow Fever alone they cannot take, and how is this to be explained, but upon the obvious fact of that disease being a seasoning fever of malignant type peculiar in a great degree to newly arrived Europeans, the product of high temperature and unwholesome locality.

“In these our latitudes, ‘cold and fatigue, and sorrow and hunger,’ under circumstances of accumulation, will generate fever everywhere; but every region, every climate, will exhibit its own form of fever. With us it is Typhus; in the warmer countries of Europe, Remittent; in the upper Mediterranean, Plague; in the Antilles and Western Africa, Yellow Fever; this last being restricted to particular localities, temperatures, and elevations. While Typhus Fever goes out when you enter the tropics, it is there that Yellow Fever commences; the pure epidemic of a hot climate that cannot be transported or communicated upon any other ground. Places, not persons, constitute the rule of its existence. Places, not persons, comprehend the whole history, the etiology of the disease. Places, not persons! Let the emphatic words be dinned into the ears of the Lords of the Treasury, of Trade and Plantations, until they acquire the force of a creed, which will save them hereafter from the absurdity of enforcing a Quarantine in England against an amount of solar heat, of which its climate is insusceptible. Let them further be repeated in the Schools of Medicine, until the Professors become ashamed of imbueing the minds of the young with prejudice and false belief, which, should they ever visit warmer climates, may cause them to be eminently mischievous in vexing the commerce, and deeply and injuriously agitating the public mind of whatever community may have received them.”

It may be interesting here to state, that while Yellow Fever thus differs from Cholera in circumstances which greatly influence the extent of its range, all observers are agreed that there are two conditions of the system in which there is a remarkable resemblance between the two diseases. The first of these relates to the condition of the blood. It is the general opinion that in Cholera a pecu-

liar poison enters the blood, in consequence of which this vital fluid becomes disorganized and a fatal discharge takes place of watery fluids derived from it. In Yellow Fever, the blood appears to be equally poisoned and disorganized, but in this disease it is the more solid portions, and particularly the red particles, that are poured out from the system.

“In Yellow Fever,” says Dr. Fergusson, “the crasis of the blood is as much broken down before death, and its vitality destroyed, as it could be by the introduction of the poison of the serpent’s tooth; we may truly say, it is killed by the poison, and in the language of John Hunter, that ‘fatal Yellow Fever is the death of the blood;’ it wells up in floods from the mucous surface of the stomach, in the form of black vomit; it escapes from the gums, the nostrils, the eyes, the ears, even the skin itself, in any or every part, and after death it will be seen to have lost all the character and composition of blood, being found in its vessels like the lees of port wine, or the grounds of coffee.”

Dr. Gavin states, that cases occur during the Yellow Fever epidemic now prevailing in Demerara, some of which have come under his own observation, in which the symptoms simulate those of Cholera so closely that, in a Cholera epidemic, they would be called Cholera.\*

The second point of resemblance between Yellow Fever and Cholera is the similar condition of the brain. In Cholera the clearness of the intellect, and the calmness of the mind up to the last moment of life present a striking contrast to the mental dulness and cloudiness, the delirium and ultimately the total unconsciousness of Typhus: the functions of the brain in Yellow Fever are generally retained in the same perfect manner as they are in Cholera.

“Self possession and courage,” says Dr. Fergusson, “ordinarily characterise the disease. I have seldom known any one who could not give clear directions in regard to the disposal of their affairs, or fail to conduct themselves with resignation. It is not always, nor often, a painful disease in its termination, and the vomitings are never, I may say, attended with pain. A gallant officer said to me, ‘You see I am posting to the other world, and you cannot prevent it, but I am as easy as if I was in a post chaise.’ Sir James Leith, Governor-General of the Windward and Leeward Colonies, whose chivalrous heroic

\* See Appendices pp. 145, 298, for exceptional varieties in other epidemics.

character graced and adorned the military profession, when he contemplated that harbinger of death, the black vomit, pouring from his stomach, on the evening preceding his death, rose from his couch in full possession of all his acumen, to execute some legal deeds of importance, declaring at the same time, in reply to my dissuasions, he could with equal facility have drawn out a plan for military operations. Lieutenant Wright, one of my earliest patients at Port-au-Prince, St. Domingo, on the fourth day of the fever rose from his bed in perfect possession of his senses, dressed himself correctly, and went into the market-place accompanied by myself, where he spent some time purchasing fruits and other things, returned to his barrack-room, where he shortly expired in a torrent of black vomit. Lieutenant Mackay, of the Quarter-Master-General's department, Cape St. Nicholas Mole, on the day of his death, was up and dressed on the sofa, with books and papers before him at ten in the morning, passing jokes of comparison between his own dingy complexion, made so by the disease, and that of his mulatto nurse; at two he expired in the same way as Lieutenant Wright. A sailor on board the flag-ship of Admiral Harvey, with whom I was taking a cruise for my health in the year 1816, was attacked with Yellow Fever, but he would not acknowledge he was ill. He was all the time up and dressed one of the earliest in the morning, saying that there was nothing the matter with him, and that he would go back to his duty the next day. He owned he was sick at stomach, and that the vomitings were dark-coloured, but that was from the red wine negus they had given him to drink, or the coffee which the cook had spoiled, and therefore made him sick; and in this persuasion instead of going to his duty, he died on the following day. Similar cases are given by Dr. Rush in his inquiries into the Yellow Fever at Philadelphia."

"The patient," says Dr. Blair, "dies with intelligence unclouded, and his muscular strength but little impaired, telling you he is getting quite well, or as a poor dying Irish sailor expressed himself, 'ilignant this mornin.'"

"Often," says Dr. Davy, "extreme danger exists when the patient considers, himself all but well, and when the pulse is little different from that in health."

Yellow Fever further resembles Cholera in the slightness of the apparently determining cause which often ushers in a fatal attack.

"Sometimes," says Dr. Blair, "the determining cause seemed of the slightest description: the shock on the stomach by an ice-cream or glass of iced punch, or the indigestion of an unripe orange, would occasionally set the train of symptoms in motion.

It seemed at one time as if those resident in the infected districts circulated the poison habitually through their system; that old residents had in an eminent degree the power of eliminating it and keeping its presence latent — had a tolerance of it; but that new comers, and particularly those of florid complexion and rigid fibre, were constantly, in reference to the presence of the virus, in a state of *tottering equilibrium*; so that in them the slightest unfavourable impulse to the balance—the lowering of the vital powers by fatigue, the suppression of any of the depurating secretions, a shock to either of the nervous centres, or the depressing emotions—were sufficient to excite the latent poison. \* \* \* \* \*

In those cases where the immediate attack was referable to a slight determining cause, the disease always exhibited its utmost violence. An instance of the determining effects of the depressing emotions is well exhibited in the case of Mr. Rankin, an old colonist. Mr. R. had been in good circumstances up to a short period before his death, in 1842, and although living in the midst of the worst district of Water-street, he retained robust and uninterrupted health. About 1842 he became involved in the general mercantile distress then prevalent in George Town, in fact, he was utterly ruined. Moreover, immediately before his illness, he became particularly depressed by what he supposed to be treachery on the part of one of his friends. The malaria then produced its deadly results on him. He died of black vomit, after having passed unscathed through the pestilence of the previous years. Even fretting and temporary chagrin have been the determining cause of a fatal invasion; an instance, the Notes of which are now before me, is in the case of Dr. Leitch, late Surgeon to the 'Arabian' emigrant transport. On Saturday morning, the 22nd September, 1843, the proprietor of a plantation on Wakenam engaged to call for Dr. Leitch, to carry him for a few days to his estate. The gentleman neither came at 11 o'clock, the hour appointed, nor sent any message of explanation. Dr. Leitch, all prepared, waited hour after hour on the quarter-deck (under awning) till evening, and was much fretted by the disappointment. That night he got the Yellow Fever, and died on the sixth day of his illness, with black vomit. In the same page of my Notes with Dr. Leitch, I find a case of death from Yellow Fever, the determining cause of which also was distress of mind. It was a mulatto, of the name of Felix Theome, a native of Martinique, whose goods had been distrained in Berbice, by a Commissary of Taxation, on the plea of their being smuggled. As was to be expected in a constitution of the tropical regions, his system struggled hard against the strong tendency to death. He died on the 12th day of his illness, and four days after black vomit had ceased.

"The mental emotions had not only the effect of developing the disease in the susceptible, and those who had been exposed to

the morbid localities, but also played an important part in the procession of the symptoms, and on the result. During the progress of the epidemic it was discovered that if a sailor, affected with fever, happened to be brought to the hospital when the hearse was present, the worst prognosis was to be formed. The intelligence of the arrival of the hearse had also the most injurious effect on the sick and convalescent within the wards. So much was this the case that a new dead-house had to be built out of sight of the hospital, and the approach of the hearse so managed that its visits were unknown to the patients. As fear and grief and the other depressing congestive emotions acted as auxiliary causes, so, on the other hand, did confidence and hope obviate the tendency to death; and, in accordance, moral courage and exaltation of feeling acted as the most powerful *adjuvans* of treatment."

Yellow Fever, like Cholera, breaks out where there is not the slightest ground for supposing that it is derived from a foreign origin, where the strictest inquiry fails to trace it to an infected source, and often where communication with infected persons or articles is impossible. Take for example one of the most recent instances; the outbreak of Yellow Fever in Brazil in the autumn of 1849, a country in which it is supposed rarely to have occurred; and yet, if the disease be capable of importation as an epidemic, it must have been constantly carried from the West Coast of Africa to Brazil by the numerous cargoes of slaves which, it is notorious, were smuggled into the latter country, closely packed together in slave ships, under circumstances most favourable for preserving the virus of a contagious disease. It is stated by masters of ships that, during the prevalence of the late epidemic, though they came direct from Europe and held communication with no vessel of any kind on their passage, Yellow Fever made its appearance on board their ships as soon as they approached the coast of Brazil, and came within the influence of the land breezes. Here communication with infected persons or articles was impossible, just as in the Epidemic Cholera of 1848, communication between the first persons attacked in London was proved to have been impossible. See Report on Epidemic Cholera, pp. 14, 23.

Dr. Gavin states, that when Yellow Fever broke out in George Town, Demerara, at the end of 1851, cases of the disease had occurred in the town before any ap-

pearance of it among the shipping; and he records the fact that

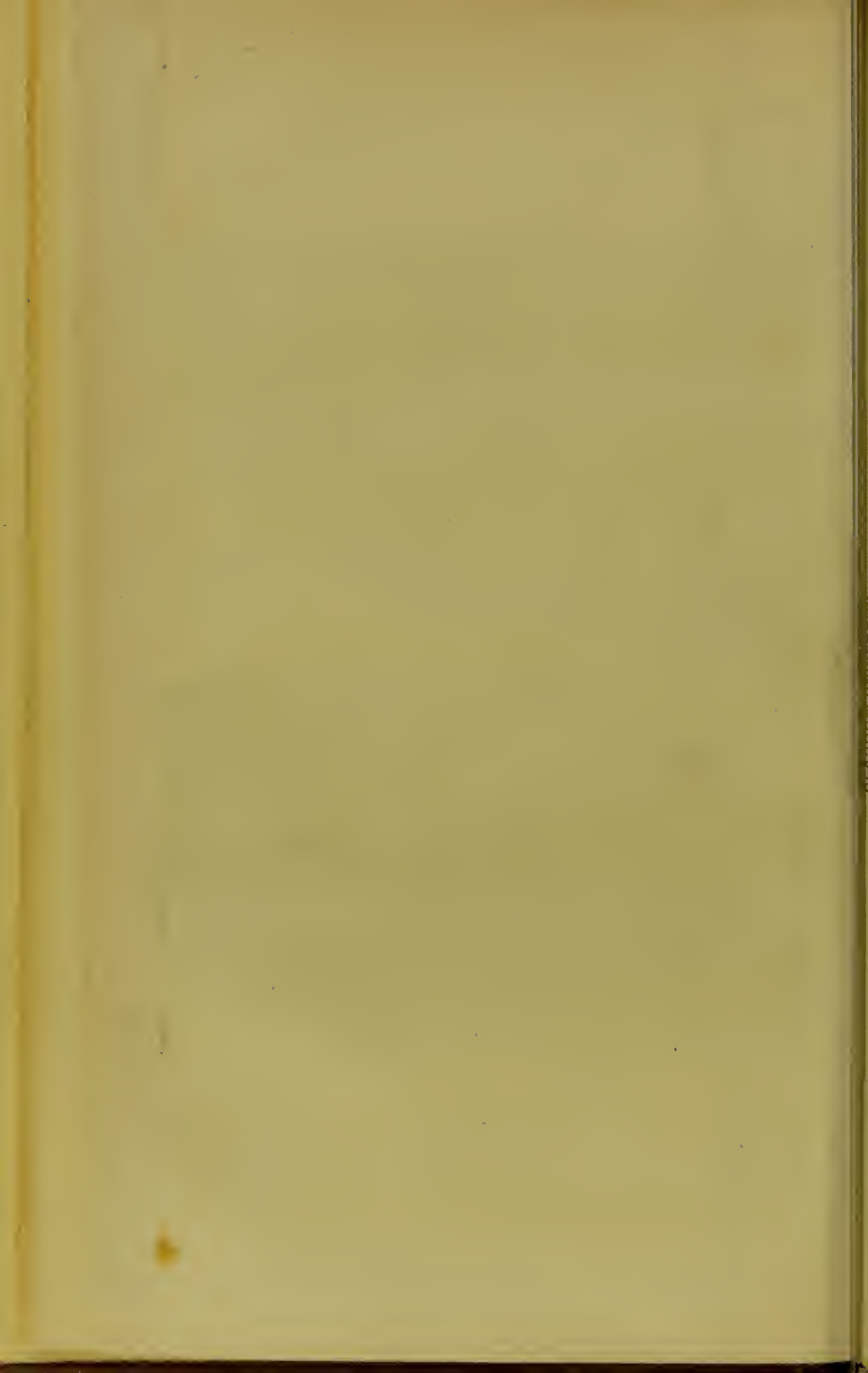
“Some seamen arriving from Europe were attacked with Yellow Fever on nearing the coast and getting into the muddy water some days before their arrival in harbour, and who had had no communication with aught else but the skies and seas and their own vessel.”

In like manner Dr. Gillkrest states that in the Yellow Fever epidemic of Gibraltar great numbers of soldiers were attacked who had never come, and who could not possibly have come in contact with an infected individual; that the men had no opportunity of mixing with the civil population or entering houses which might have contained sick persons; being during more than three months of the period encamped outside the walls without being allowed to enter the town, except when on duty.

“The mass of those attacked,” he says, “merely marched to their guard-houses. to which, certainly, the inhabitants were not in the habit of resorting, and where the soldiers, as certainly, came in contact with no persons suffering from Yellow Fever. Arrived at his post, a soldier was placed as sentry, not in the midst of a dense population; not with people about him, from whom disease might be transmitted; nothing of this occurred. The guard in charge of the ruins of Pompeii is not in the midst of a deeper solitude, than were often the soldiers at Gibraltar, as they inhaled the ‘death-blast,’ in districts abandoned by the inhabitants. The men were far removed from contact with sick people—far out of hearing, far out of sight.”

Mr. Amiel, surgeon of the 12th regiment, confirms this testimony.

“I may add,” he says, “that when so many new cases occurred in the regiment after it had resumed the town duties, the men composing the guards were marched directly from the Neutral Ground to the Guard-houses, mostly by the Line-wall, and avoiding the streets as far as possible; those in guard-houses had no communication whatever with any description of inhabitants, and still less with the sick, or articles belonging to them; they were marched to and from the posts of the guards without being allowed any improper intercourse, and ultimately they went back to the Neutral Ground, with the same precautions, and in the same orderly manner as they had come from it; thus rendering it impossible to trace, to any contagious sources, the malady which



member of the Board of Health established in the garrison, in 1813, says :—

“The disease did not spread from any focus, but broke out in fifty different places at once.”

Other instances of the simultaneous outbreak of Yellow Fever, in different and distant localities without communication, might be multiplied indefinitely.

In Yellow Fever epidemics as in those of Cholera there appears to be a gradual local development of the disease, the outbreak of the epidemic being preceded by individual or sporadic cases, in greater or less number.

The medical officers resident in Gibraltar concur in stating that individual cases, identical with those of the epidemic of 1828, had been observed by them every year, from 1816, up to the breaking out of the epidemic. Before the commission appointed to inquire into the origin of this epidemic, Messrs. Fraser and Wilson, medical officers attached to the Civil Hospital, and both having extensive private practice, depose to their having annually witnessed cases of fever identical with the epidemic.

Mr. Wilson declares that he treated about 200 cases of the late epidemic: that he had seen the same disease in Jamaica, in Carthagen, and in the Havannah, and that he had in the Civil Hospital at Gibraltar generally every year, from 1815 to 1828, when that epidemic broke out, met with cases of the same disease accompanied by the same symptoms, and terminating in death under the same circumstances as cases of the late epidemic. He adds that he had carried from the West Indies to England a specimen of the black vomit, and speaks positively to the identity with it of the black vomit which he had observed in sporadic cases at Gibraltar between 1815 and 1828.

Mr. Fraser stated that he had treated about 200 cases of the late epidemic, and that he had seen many, perhaps 500 or 600 more; that he had been surgeon to the Civil Hospital about five years, which situation necessarily forced him to pay attention to the general health of the place, and that he had directed his attention particularly to the subject of fever; that during the five years of his

residence, and previous to August 1828, he saw between 40 and 50 cases which he is inclined to identify with the epidemic; that he saw no difference in symptoms between these sporadic and the epidemic cases, even those with the black vomit; and that an analysis of the black vomit had been made in the Civil Hospital long before the breaking out of the epidemic.

“It was evident,” says Dr. Smith, “that there was a febrilizing influence present in our atmosphere the whole of the year 1828, as was manifest from its effects, for scarcely a month passed without cases of sporadic Yellow Fever occurring.”

“The occurrence of sporadic cases of this disease in years not epidemic,” says Mr. Howell, “is confirmed by the general course of events at the beginning of the late epidemic. The alleged first cases (those of Fenic’s children) struck their medical attendant with no surprise or alarm: if these cases had presented appearances which he had not been accustomed occasionally to see—even what is described as ‘the fatal symptom of black vomit’—he would naturally have been astonished and alarmed at the occurrence of a disease foreign to his practice. In the earliest cases, no medical man who had not witnessed an epidemic denounced the appearance of an unknown disease unusually rapid in its progress, or marked by any unusual symptoms, nor did any medical man who had witnessed an epidemic suddenly proclaim to the community his having for the first time since the year 1814, seen a case identical with that disease, and warn the public of the recurrence of a malady from which they had been for fourteen years exempt. On the contrary, judging of men’s real opinions by the safest criterion, their own acts, I find that the circumstance which caused experienced as well as inexperienced medical men to announce the presence of the Yellow Fever epidemic was not the occurrence of one, of two, or of more cases, but the gradually increasing number of cases of the same disease, from day to day, in the same district of the town;—that it was not by anything unusual in the nature of the disease, but by something unusual in the number of cases, that the advent of a Yellow Fever epidemic was ascertained.”

An attempt was made before the Commission of Inquiry to discredit the fact that these alleged sporadic cases were really cases of Yellow Fever. Of this attempt one of the Commissioners, Mr. Howell, gives the following account:—

“Mr. Fraser, in support of his opinion that the disease was indigenous, presented to the Board a list of thirty-nine cases of

Yellow Fever, recorded in the case-books of the Civil Hospital as having occurred in years when no epidemic prevailed. To counteract this, evidence was adduced to show that some few of the patients in these sporadic cases had suffered the fever in some epidemic season, whence it was intended that we should infer that the cases presented by Mr. Fraser must have been cases of some other disease.

“ Had this been satisfactorily proved, the credit of the case-books, and the testimony of Messrs. Fraser, Wilson, Dix, Browne, and Gillice, might have been in some measure shaken, inasmuch as it had been decided by the Report of a Medical Commission that ‘one attack of Yellow Fever preserves the individual from a second.’

“ In opposition, then, to Mr. Fraser’s list, the hospital patients themselves in some instances came to depose to their having had the Yellow Fever in some epidemic year; and in other instances, when the patient himself was dead, or not to be found, his relatives, or some one who said he knew him, gave evidence to the same effect. No medical evidence whatever was produced; but the bare assertion of a bricklayer, a labourer, or a journeyman butcher (this was the occupation of Sabah, as deposed by himself, though it does not appear upon the minutes), was given as evidence capable of outweighing the professional opinions of the medical attendants of the hospital supported by the entries in the case-books. On a professional point such impeaching evidence would be worthless under any circumstances; but on this particular occasion the impropriety of receiving it as conclusive is aggravated by a remarkable instance of inconsistency.

“ The Report of the Commission, which is made the test of the hospital cases, contains the following sentence:—‘ Four physicians had attended in 1828, some patients who said that they had had the Yellow Fever in one or other of the former epidemics; but as the symptoms of the first attacks could not be stated, the Board could not take these cases into consideration.’ Now as it is certain that no symptoms were stated to the Board by the journeyman butcher, or his colleagues in their attempt to shake the credit of the hospital cases, it would seem to be particularly unfair to admit, as sufficient to overturn these cases, such evidence as would have been rejected by the Commission whose Report is made the foundation of this line of inquiry.

“ The same evidence, to establish the same fact, cannot be good in one place, and bad in another. If good against Mr. Fraser’s cases, the Report of the Commission, which excluded such evidence as bad, must of course be itself worthless, as being founded on insufficient testimony, and then the fact found by that Report cannot be assumed as a test to try any

question whatever; but if such evidence be bad, and the Medical Commission were right in rejecting the loose assertions of ignorant men, unsupported by any specification of symptoms, or by the testimony of a medical attendant, then the evidence of Sabah and the rest must be rejected by *us*, and the genuineness of the Civil Hospital sporadic cases stands unimpeached.”  
—See Certificate, Appendix I., p. 155.

That Yellow Fever constantly occurs as a sporadic in those parts of America in which it occasionally prevails as an epidemic, is proved by the general testimony of the medical men of such districts. Without multiplying examples, it may suffice to cite the statement of Dr. Hort of New Orleans, who says,—

“ Until within three years past, during which time the fever has been rather sporadic than epidemic, the Yellow Fever prevailed with regularity as an epidemic every other year, but there were always sporadic cases.”

Dr. Arbuckle, of Pernambuco, writing to his brother, states that a Brazilian lady died of Yellow Fever in that city on the 17th August, 1849, whereas the epidemic of that year, alleged to have been imported from Bahia, did not appear until December.

Dr. Gavin states that in the recent Yellow Fever Epidemics of French and Dutch Guiana, as well as in those of British Guiana, indubitable evidence of the presence of a Yellow Fever influence was present, two and even three months before the cases became so numerous and decided as to warrant the declaration of the existence of a Yellow Fever Epidemic.

Among the concluding remarks appended by Mr. Watson, surgeon of the Naval Hospital, Port Royal, Jamaica, to an elaborate return of all cases of fever admitted into that establishment from 1815 to 1849 inclusive, occurs the following:—

“ We constantly meet with sporadic cases of the most fatal character at times when the general health of the community is excellent.”

In Yellow Fever, as in Cholera, when the disease breaks out in a family, it does not in general spread through the family, but attacks only one or two members, the rest

escaping, even those in the closest attendance on the sick ; and when, as does occasionally happen, several individuals in the same family are attacked, it is found on inquiry either that the epidemic was general in the locality in which such family resided, or that the individuals attacked had gone into a locality in which the disease was prevalent.

“ The numerous instances,” says Mr. Amiel, “ of two, three, or more persons having been attacked in the same house, at the same time, and even at the same hour, and that general susceptibility which so rapidly pervades all ranks, show, not that the disease has the property of spreading from person to person, but rather that it is produced by a general cause, to the influence of which they have been simultaneously exposed.”

In Yellow Fever, as in Cholera, instead of the disease spreading from house to house in the district or locality which it invades, it is often confined in the most remarkable manner to particular houses in the same street, to particular houses on one side of the street, and even to particular rooms in the same house. Dr. Fergusson states, that he often observed the influence of the infection to be so limited, that one story of a house, or one section of a ship would be strongly affected, while the other parts of the same tenement remained healthy. It is recorded that during the epidemic which prevailed at Barbados in 1838, of 36 individuals residing in a building appropriated as quarters for the officers, 28 were attacked of whom 10 died ; whilst of the whole regiment residing in the soldiers barrack, at so short a distance as fifty paces from this fatal spot, only 30 cases occurred and none died. On other occasions it was observed that in certain barracks and hospitals, the very diagonal of particular apartments afforded an accurate demarcation of the safe and unsafe position of beds ; and in ships the disease is often confined to men whose berths are on a particular side, or in a particular part of a ship. The causes of these attacks may be ascertained by careful examination.

“ Particular stations,” says Mr. Amiel, “ as North and South Flat Bastions, South Port, and the Convent Guards, were found to give rise to more concentrated forms of the fever than others, and the soldiers were attacked in greater number on these than on the other guards ; and it is worthy of remark that, with one

exception, these posts were on the line, or immediate vicinity of the drains leading from 24th district.

"The line-wall, skirting the sea-beach, was also discovered to be a very mortal station. Few sentries planted there, if any, escaped an attack; and Colonel Payne, commanding the artillery, who remained in the town from the commencement of the fever until his death in the month of December, lost his life by visiting the locality at night. The known unhealthiness of this station caused many of the sentries to be withdrawn from it at night, and the consequence was some depredations were committed on the great guns; and the Colonel, in his zeal to detect the offenders, perambulated this district for three successive nights, and on the third was suddenly attacked, and expired on the fourth day of his illness.

"The examples of people enjoying immunity from situation alone, though in constant communication with the sick, might be multiplied without number. In the same house even, families occupying the upper stories were known to escape, while those on the ground floor suffered severely.

"In Bossano's house at Rosia, for example, many individuals in the upper story escaped the disease, while every person in Belasco's family, living directly under them, and who had not passed the fever, were attacked. In another house in the south, 16 individuals died on the ground-floor, and not one was taken ill on the upper.

"On the other hand, there were localities to which the disease never spread, and in which it seemed incapable of existing, as the Neutral Ground, Europa Flats, and the Bay. Our late epidemic could not be propagated in any of these localities."

In Epidemic Cholera the most rigid seclusion affords no protection; so in Yellow Fever epidemics, instead of increasing, it appears rather to lessen the chance of escape.

"In the army of St. Domingo," says Dr. Fergusson, "it was notorious that those who were the most careful to seclude and shut themselves up, were ever the first to be taken ill, and the surest to die; and during the Yellow Fever epidemic of 1816, at Barbados, I have recorded remarkable instances of the same, both from my own observation, and that of others."

It was the same during the Barcelona epidemic of 1821 and the Gibraltar epidemic of 1828.

"Families," say the Committee of Physicians of Barcelona, "who isolated themselves in their houses, employing the most exact precautions for avoiding external intercourse and communication, did not by such means preserve themselves from the malady."

“Many families in Gibraltar,” says Dr. Smith, “secluded themselves without escaping the disease, and I myself witnessed numerous instances of this.”

“Shutting up houses,” says Mr. Amiel, “burning furniture, and prohibiting intercourse with the sick, have had no effect in checking the progress of the disease.”

When the alleged cases of security afforded by seclusion come to be examined, it is generally found that they are without any real foundation. Of this the following may be cited as examples. In speaking of the Yellow Fever epidemic which prevailed at Gibraltar in the year 1813, Sir William Pym states:—

“Of 500 persons confined to the dock-yard during all the time of the sickness, there was not an instance of one of them being attacked.”

Again, he says:—

“The labourers belonging to the naval works have been kept in strict quarantine in the dock-yard, very near the spot where the disease showed itself in 1810, and if there is a situation in Gibraltar favourable to the generation of marsh miasmata it is there; and in 1804, it shared the fate of the other parts of the garrison; yet those people this year have continued healthy, as well as another party of inhabitants, who established themselves in Camp Bay, and cut off all communication with the infected.”

Dr. Gillkrest quotes from Dr. O'Halloran the following comment on this passage:—

“The perusal of the foregoing quotations in the work of Dr. Pym struck me forcibly on my arrival at Gibraltar in the present year. I thought the immunity of the dock-yard from fever in the year 1813 a singular circumstance, and one which strongly operated against the doctrine which I am inclined to embrace. I had not doubted the assertions of Dr. Pym and Mr. [W. W.] Fraser; for, from their rank in the service, the one being at the head of the Health Office in London, and the other at the head of the Medical Department in Gibraltar, it was reasonable to expect information of authenticity, for their opportunities of attaining it exceeded that of others. It happened, however, by accident, that a medical gentleman, who saw the epidemic of 1813, observed, in the course of conversation, that fever prevailed to some extent in the dock-yard that year; and that, by an application to Mr. Buck, who was secluded with the others, and who is now the superintending officer in charge of the establishment, particular and authentic information might be obtained on the subject.

“ I applied to Mr. Buck, and the information which I have obtained from him and his head clerk is the following ; it may be depended upon as officially correct.

“ The number of persons secluded, according to Mr. Buck’s account, who examined the books in my presence, amounted to 170. Dr. Pym makes it 500.”

Dr. O’Halloran then quotes from the records of the dock-yard the names of the persons who were there attacked with Yellow Fever, together with the names of those who died of that disease, amounting in all to 24, of whom 16 recovered, and 8 died, or 1 in 21 of the persons secluded. The dock-yard authorities concluded the certificate, which they gave to Dr. O’Halloran, in the following words :—

“ The truth of the assertion as to deaths having taken place there (the dock-yard) cannot be doubted, and the list of sick falls far short of the numbers affected ; but the dread of being sent to the Lazaretto, and being separated from their friends and relations, caused the sufferers to conceal the disease in many instances.”

Dr. Gillkrest confirms by his own personal examination the correction thus given by Dr. O’Halloran.

“ During my residence at Gibraltar,” he says, “ I had ample means, by referring to the official authorities at the dock-yard, of confirming the assertion of Dr. O’Halloran that several cases of the fever prevalent in 1813 occurred there, as well as some deaths ; but to my utter astonishment I found Sir William Pym’s statement repeated in the second edition of his work (page 34) printed in 1848, though he had been twice in Gibraltar after the error had thus been publicly exposed.”

In like manner Sir W. Pym, in enumerating several private families who escaped an attack in 1828 by cutting off all communication with the infected, places in this number the family of Judge Howell ; but Mr. Howell authenticated before the Board of Inquiry, of which he was a member, the evidence which he had previously given to the Anglo-French Commissioners, which was to the effect, that he had not secluded himself ; that he was in the constant habit of receiving people on business into his house—paymasters of regiments, for example, to swear to their pay-lists, and many others ; and that Mr. Amiel, Surgeon of the 12th Regiment, had been in the habit of

visiting his family often directly after leaving the wards of the hospital.

During the late Cholera epidemic, the most striking examples occurred of the sudden cessation of the pestilence on the removal of the population, whether sick or healthy, from the infected localities; in like manner, the history of Yellow Fever epidemics abounds with similar instances.

Dr. Gillkrest gives numerous examples of this in Appendix No. 1, page 202.—Dr. Smith states, that on the outbreak of the disease in the 12th Regiment, this corps was immediately encamped on the Neutral Ground, and not a single fresh case occurred until the regiment again resumed night duties in the town. Mr. Amiel, surgeon of the regiment, confirms this statement in the following words:—

“The 12th Regiment was removed to the Neutral Ground on the 5th of September, after having sent four cases of fever to the 12th Hospital, two of which died with the dreadful symptoms of black vomit; but from the period of their encampment until the 25th of September, the soldiers having had no duty to perform in the town, no fever cases appeared among those in the camp, while several orderlies attached to the different departments which remained within the garrison were attacked. It was from the 25th of September, when the regiment had resumed the town duties, that is to say, when the soldiers came to breathe the pestilential exhalations in their stationary sources, that the admissions became numerous and the increase of the disease alarming.”

The disease also broke out early in the 23rd Regiment, quartered in Rosia barracks. The regiment was encamped at Europa Flats, after which not a single case occurred among the men, who never left the camp.

“The most signal success,” continues Mr. Amiel, “has followed the removal from the impure atmosphere of the Rock of those who appeared most susceptible of the fever, as was done in 1813 with many thousand inhabitants, in 1814 with the sickly regiments, and during the late epidemic with the whole population of the 13th and 24th districts.

“In 1814 the Regiment of Dillon, which had arrived in the month of May, was quartered, at the breaking out of the epidemic, in the Blue Barrack, near the Moorish Castle. A great number of men took the fever and soon died; in consequence of which the regiment was encamped on the Neutral Ground, and immediately the fever stopped.

"The 8th battalion 60th Regiment, arrived from Cadiz in August of the same year, and encamped on the Governor's meadow in a healthy state. Shortly afterwards they went into the Cooperage Barracks in town; the fever soon broke out amongst them, and both the officers and men suffered severely. They were sent back to the encampment, and the disease instantly ceased as if by magic."

Mr. Melvin states, that on two occasions he succeeded in putting an immediate stop to severe visitations of Yellow Fever among the troops, by removing the men from the infected locality, and that the effect of the removal on the sick was instantaneous.

"Some of the cases," he says, "were bad; one of them at the time was bringing up large quantities of black vomit, and had much hæmorrhage from the gums and nose, with an exceedingly cadaverous and disagreeable smell emanating from his body; in fact, he in every way appeared to be quite hopeless. However, to my delight, this case, as well as every other, perfectly recovered, which most assuredly would not have been the case had not the change of situation been made. And to my further delight and comfort, I had not a single servant attacked with Yellow Fever, nor did I lose a single case from it while I occupied this old hospital; but I had cases of regular Yellow Fever among some of the families in the barracks. During the period I have been speaking of, I attended the sick of the white troops entirely myself. With regard to the ground about this old hospital which I had the sick taken to, it was well drained, and, above all, it was out of the influence of the new marshy and muddy ground so very close to the new regular hospital."

Dr. Spence states, with reference to the fatal spot occupied by the officers in the garrison of Barbados, in 1838, where out of 36 residents, 28 were attacked with Yellow Fever; it was recommended that the quarters should be evacuated forthwith, and forthwith the fever stopped, for after the adoption of this measure not one case occurred.

"When Yellow Fever makes its appearance in a particular spot," says Dr. Davy, "removal from that spot is the true measure of security; sometimes removal to a short distance, as of a few hundred yards, would appear to be sufficient; but, if it be practicable, removal to a greater distance is desirable, and to ground in its character different from that where the disease originated. Late experience in Barbados proves the propriety of this. When Yellow Fever broke out in the 88th Regiment, occupying the lower bar-

racks in the garrison of St. Ann, the encamping of them on ground close to the higher barracks, in the same garrison occupied by the 7th Royal Fusiliers, who were then free from fever, and altogether escaped it, was successful. The same result occurred, for a time, when the disease appeared shortly after in the 66th Regiment, who followed the 88th, in the lower barracks: their removal to the higher ground at first appeared to be successful, but only for a while. Ere long the fever made its appearance in the upper barracks, then occupied by the 72nd Regiment, and indeed appeared more or less throughout the garrison. With the experience I now have, and the strong conviction in consequence, I am satisfied that in the instance of this outbreak of fever, whether in the 88th, 66th, or 72nd, the recommendation on the part of the medical officer, when consulted by the general officer commanding, would have been best given in conformity with the above, to remove the regiment attacked, not from one part of the garrison to another, but to a greater distance, and to a situation altogether different from that where the disease had made its appearance. Having, in my capacity of Inspector-General of Hospitals, been the medical officer consulted on the occasion referred to, I have the less hesitation in making these remarks. Whether, in a military point of view, such a removal of the troops from the garrison to a distance could have been effected with propriety is altogether another consideration. Life, it must be remembered, in the army, must always be held subordinate to duty; and it should be remembered, too, that there are many instances on record where a removal to a very short distance has been effectual. With the medical officer the preservation of life is the only consideration; with the general officer, duty is the first consideration."

The testimony is uniform, that the removal of the sick takes place without transmission of the disease.

Dr. Chervin says—

"In 1802, Admiral Gravina caused 500 patients with Yellow Fever to be disembarked at Cadiz, who were taken to the hospital St. Juan de Dios, and treated therein, without having communicated their malady to any person."

The same author shows, by the concurrent testimony of the medical men of upwards of thirty cities and towns in North America, that the arrival in these places of persons labouring under the most malignant form of Yellow Fever was followed by no communication of the disease.

Dr. Ashbel Smith, describing an outbreak of Yellow Fever at Galveston (Texas), says—

"The sick have been in many instances removed from the infected district to healthy sections of the city, and in no case, as far as I can learn on careful inquiry, have the attendants or inmates of the houses contracted the disease."

At the commencement of the Gibraltar epidemic (1828), the population of the districts first infected, consisting of about 4,000 persons, abandoned the town by order of the authorities, and encamped on the neutral ground. They took with them their bedding and other household furniture. They took with them their sick and the families of the sick; but the disease, instead of spreading through the camp, was immediately and completely arrested. Great numbers of the poor, as soon as they had passed through Yellow Fever in the Civil Hospital and were convalescent, rejoined their families in the camp, taking with them their blankets and other personal articles, yet there is no known instance in which they communicated the disease.

"Several of the women," says Dr. Smith, "passed the night in the same beds with their husbands attacked with, and labouring under, the epidemic fever, and, besides, continued, as well as their numerous children, to use the same bedding after the men had been removed to hospital; but in no instance was the disease contracted by the wife or the children, even after that full exposure."

"Out of 92 women and 190 children, belonging to the 12th regiment," says Mr. Amiel, "who were not allowed to pass Bay Side Barrier (the entrance into the garrison), not one had the fever, though several of them slept in the same beds with their husbands labouring under the epidemic, and continued to use the same bedding."

"It has been said, that the pure air of the Neutral Ground checks the contagious property of the fever; but when the wife in the same bed came in contact with the patient scorched with febrile heat, or bedewed with copious perspiration, when she inhaled under the same tent the effluvia of his breath, how could the air, however pure, sufficiently interpose to prevent the process of contagion, and its fatal consequences?"

"From the foregoing observations, I conceive it results that the disease does not spread when the sick are removed from the impure air in which it was contracted, and likewise that, by breathing the impure air without exposure to the contact or effluvia of the sick, persons are frequently attacked; while, on the contrary, without breathing it, however exposed to such contact or effluvia, no person is attacked."

During the Barcelona epidemic of 1821, the experience was similar.

"A great number of persons," say the Committee of Physicians, "who, after passing the whole day in the capital, retired at night to their families, either in country-houses, or in the nearest villages, communicated the disease to no one, whatever were the situations of these houses; not even those who had quitted the town on the very day on which they had lost a member of their family, and notwithstanding their having taken no precautions.

"The daily traffic of carriages which had conveyed sick persons furtively introduced, or mattresses, linen, clothes, and other furniture, taken from the very focus of infection, did not transport the malady beyond the limits which had been assigned to it.

"Notwithstanding the crowds squeezed into the smallest habitations, the general panic, the heat of the weather, and the combination of many other causes well calculated for the propagation of disease, however destitute that disease might be of any contagious property, it could not be transplanted out of the city.

Soon after the outbreak of the epidemic at Leghorn in 1804, 6,000 persons left that city for Pisa; at the same time the French army removed to the same place, taking with them 180 men labouring under the disease, yet there was no propagation of the disease at Pisa.

Testimony is borne by Humboldt, that the experience is the same at Vera Cruz, where he states it is the practice to remove the sick as extensively as possible from the city into healthy districts. "Not only at Xalapa and higher up in the interior," says this distinguished observer, "but at the farm of Encero, a short distance from Vera Cruz, the disease is found to confine itself to the persons of those who may arrive with it in their systems, notwithstanding the freest intercourse with others."

During the recent epidemic in Brazil, many of the residents in Rio de Janeiro, in order to escape the disease, fled to the town of Petropolis: several of the fugitives were attacked after their arrival there, but in every instance within three or four days from the time of their departure from Rio; and no inhabitant of Petropolis who had not been to Rio, is known to have suffered, though several who did go into that city had the disease and died. "The disease," says one of the witnesses, "was contagious only within Rio itself!"

"The disease then existed at Petropolis," says Dr. Croker

Pennell, "with sufficient violence to destroy a large proportional number of lives; for it is believed that the eleven deaths which took place formed a high rate of mortality compared with the number attacked. It is probable that at least seven persons held communication with each individual who died, either as attendants, nurses, or in washing or burying him; so that we are thus presented with an instance of from 70 to 80 persons exposed to a violent infection, if such it be, and singular to relate not one contracted the disease: not a single resident at Petropolis caught the fever."

The testimony borne by naval and military surgeons is almost uniformly to the same effect.

On the outbreak of the epidemic at Gibraltar, Yellow Fever patients were freely admitted into hospital, when there were men with other diseases in the same wards.

"Yet," says Dr. Smith, "I never observed any mischief to result, although no precautionary measures were adopted. The hospital servants escaped until the disease had become general in the district where the hospital is situated. The officers, also, who confined themselves to the camp, escaped, although some of them accompanied their sick companions to hospital, rendering them every kind office, and sitting with them in the same waggon which conveyed them to hospital.

"The convalescents were discharged directly to their regiments without their *kits* or wearing apparel being submitted to any process of purification. I myself had daily communication with the camp. \* \* \* In the Civil Hospital, such was the pressure for admission into that institution, as to render it necessary to relieve the wards the moment a patient passed the *acme* of disease; accordingly, convalescents were discharged in all stages, many of them having blood exuding from their gums; and to some very poor people the soiled blankets under which they had passed the disease were given to them. The majority of these people went to the Neutral Ground, where they recovered rapidly, and where they slept with their families as usual, and mixing with whom they pleased."

"Several cases of the epidemic," reports Mr. Amiel, "were admitted into the Regimental Hospital, three of which died with the black vomit; but the disease never attacked any of the other patients, more than twenty in number, treated during the above period for other complaints, nor any of the orderlies, who had, as usual, an incessant and unreserved intercourse with the dying, and slept in the same wards."

"The epidemic," continues Mr. Amiel, "did not spread, at Europa Flats, on board of the vessels in the bay or on the Neutral Ground, when carried thither from the town; and numbers of

individuals who had the seeds of the disease about them, even in the case of their falling sick or dying there, did not communicate the malady to their neighbours or attendants. This important fact, which I had particularly noted during the epidemic of 1813, amongst the foreign recruits quartered at the Brewery Barracks, has been most forcibly exemplified during the late calamity, when neither the bedding, clothes, &c., removed from the focus of sickness, nor the continual intercourse with some of the inhabitants who daily went out to the Neutral Ground, nor the numerous convalescents directly sent thither from the Civil Hospital, often in an early stage of their recovery, produced a solitary case of the disease beyond the gates of the garrison among that numerous population who had fled thither, and who frequently were in confined habitations, when the heat of the weather, the depression of spirits, the melancholy scene which they witnessed, and, I may add, for many of them, privations and hardships, afforded the most favourable means for the propagation of the disease, had it been of a contagious and communicable nature."

"During the late epidemic of British Guiana," says Dr. Blair, "the Yellow Fever cases in their worst forms were never separated from other patients in our hospital wards. Such a thing was not deemed necessary, and never thought of. They were classified with acute cases. Our hospital nurses never got infected, although in the closest connexion with the sick, and often smeared with their ejections; and these nurses were generally German and Portuguese immigrants. The resident surgeons, dispensers, and stewards, were all susceptible subjects, and, with one exception about to be named, escaped without an attack. Mr. Bell, the first dispenser of the Seamen's Hospital, then lately from England, spent several nights in Water-street attending on a sick friend. Mr. Huddleton got Yellow Fever, and died the night he was gazetted to his appointment, and never did one day's duty at the hospital. Certainly his friend had the same disease, but Mr. Bell caught it, as Mr. Huddleton had caught it, in Water-street. The way to give a Yellow Fever nurse the Yellow Fever, was not by bringing him in close contact with the sick, but by discharging him or her from the hospital. After knocking about town for a few weeks, and getting into the malarial districts, they would, it is likely, be brought to the hospital as Yellow Fever patients. Several nurses, discharged for bad conduct, suffered in that way. Dr. Bonyun, then one of the resident surgeons of the Seamen's Hospital, and not long from Europe, slept continuously in the Seamen's Hospital while it was crowded with Yellow Fever, without suffering from the disease, and without fear of contagion.

"In December, 1843, the mate of the 'Matilda Luckie' was admitted with the gravior form of the disease, and of a low type, of which he died. His bed was in a sheltered corner of Ward

No. 2, and had mosquito netting all around. Into this bed a seaman, named Burton, who was admitted for disease other than Yellow Fever,—slight indisposition—was put for several days without any infection of any kind following. Neither was such an experiment deemed hazardous to the subject, nor objectionable, except on the score of cleanliness. Experiments made by me on the muco-purulent looking matter which frequently exudes from the eyes in the late stages of Yellow Fever, applied to healthy conjunctivæ, showed that though slight ophthalmia followed, no Yellow Fever contamination was the result. Many ships lost 'hands' after their departure from port, and were obliged to put into Barbados and other islands for additional men; but we never heard of any spread of Yellow Fever among the islands in consequence. None of the Georgetown medical practitioners suffered from the Yellow Fever except Dr. Fraser and myself. The disease picked out the new comers of an establishment, and no reluctance was felt, either by friends, or relatives, or acquaintances, to perform any service for the sick."

On a spontaneous outbreak of Yellow Fever on board Her Majesty's ship "Bedford," when in Gibraltar bay, there being at that time no fever in the garrison, 130 sick were landed and sent to the hospital; eleven died, and others were left dangerously ill on the departure of the ship. The disease did not extend beyond the crew.

In the year 1830 a similar outbreak of Yellow Fever attended with black vomit, occurred in Her Majesty's ship "Blossom," in Belize harbour: forty-eight cases were received into the military hospital there, and two officers and eight men died. The disease did not extend to other ships or to persons on shore.

Mr. Hartle, deputy inspector general of hospitals, who served in the West Indies during a period of more than 30 consecutive years, states, that he has on several occasions witnessed the importation of Yellow Fever cases of the most malignant character, amounting in all to 107, but that he never observed in a single instance the communication of the disease to any individual.

Mr. Mortimer, principal naval medical officer at Barbados, says:—

"We have never heard of an instance of its communication to patients at the several naval hospitals whilst under cure for other complaints, though such patients have never been interdicted, on the contrary, encouraged to offer any additional aid for the greater comfort of their suffering brethren."

“At Barbados,” says Dr. Fergusson, “our hospitals of late years have been in a regular course of importation of the Yellow Fever from the navy; but not even inoculation has been able to produce the disease upon any member of the hospital corps, by whom I may truly say that the sick have been received with open arms; for the anti-social doctrines of ideal contagion are not preached among us here to the prejudice of duty and humanity.”

Dr. Magrath, Principal Medical Officer of the Public Hospital in Kingston, Jamaica, whose opinion, derived from an experience of 34 years on the island, is of the highest value, says—

“Yellow Fever has not for several years prevailed as an epidemic in Kingston, but occasionally we receive patients suffering from it into the public hospital. Most of these cases come from coal ships. In 1848 the crew of a vessel that had a few months previously been employed in carrying guano, suffered most severely; and in 1849 the persons on board two coal ships, which, after discharging their cargo, took in some impure ballast, were attacked with Yellow Fever in its most virulent form, whilst all the other vessels in the harbour remained nearly free from the disease. The sick from these ships were placed amongst the other patients in the hospital.”

“Dr. Magrath informed me,” reports Dr. Milroy, “that nearly 20 cases of aggravated Yellow Fever were received from these two vessels, (the holds of which were found to be in the foulest state imaginable,) into the hospital, and a very large proportion of them proved fatal. None of the other inmates or of the attendants of the hospital were affected. Moreover, there was no prevalence of the disease on shore at the time.

“Dr. Dunn, another eminent practitioner in Kingston, mentioned to me, that when he was Health Officer at Old Harbour, cases of malignant Yellow Fever were every now and then occurring in the shipping there, while none existed among the inhabitants of the place.

“I may mention that so frequently was the occurrence of Yellow Fever observed to take place on board vessels in Kingston harbour when they took in the foul shingle from the shore at or near the wharves, where there is always an accumulation of filth and refuse, that of late years they have been required to proceed to the palisades on the opposite side of the harbour for ballast when necessary.”

Dr. Milroy adds:—

“Dr. Chapple, one of the Spanish medical officers of the military hospital in the Havannah, informed me that the disease is

by no means unfrequent, and occasionally prevails with great severity among the patients in that institution while the city is nearly or altogether exempt from it. The hospital is notoriously unhealthy from obvious local causes; and it is an interesting fact that, during the late visitation of cholera, the disease clung in a very remarkable manner to this building."

When the attendants on the sick in hospitals do become the subjects of the disease, it is, as has been already stated, only after the epidemic has invaded the locality in which the hospital is situated; an observation in perfect accordance with the experience of Cholera in Hamburgh and other continental towns as well as in Great Britain. During the epidemic at Gibraltar in 1828, all diseases belonging to six regiments were for several weeks received into the naval hospital, Yellow Fever indiscriminately with the rest; but none of the other patients were attacked until the fever had become prevalent in the district in which the hospital is situated.

"The danger," say the Committee of Physicians at Barcelona, "so far from being in the direct ratio of exposure (contact or intercourse with the sick) was positively (in many instances) in the reverse ratio.

"In the marine lazaretto in which from the 7th of August to the 13th of September there entered 79 sick (of whom 55 died, and 24 recovered), not one out of 32 of all classes of officers and attendants, contracted the disease.

"In the Lazaretto of the Vice-queen of Peru, which received 56 sick (of whom 39 died and 17 recovered); out of 23 persons of various classes who attended them, four only contracted the disease; and these had come out of Barcelona.

"In the Hospital of the Seminario, into which 1767 persons were admitted during the epidemic (of whom 1293 died); out of 90 attendants on the sick, three only contracted the disease, which is but at the rate of 1 in 30, *constituting a far greater exemption from sickness than was enjoyed by any other portion of the community.*

"In the General Hospital, while the fever attacked persons who had no communication whatever with the sick or their effects, the vicars, the brothers and sisters, who attended the patients with the purest charity, the physicians, surgeons, &c., remained in perfect health.

"It was not," says Dr. Gillkrest, speaking of Gibraltar, "until the residents in the southern district, where the hospital containing the sick of six regiments was situated, became affected with the noxious emanations that prevailed that any hospital

servant was attacked; and the two first attacked were the cook and the waterman, whose duties never took them into the wards. Those of our permanent orderlies who suffered were for a month in close contact by day and night with the patients before being attacked. On the removal of the hospital to the barrack at Windmill Hill, an unaffected district, not a single hospital servant or patient was subsequently attacked.

"One would naturally suppose," observes Dr. Fergusson, "that wherever a true infection existed, the hospital, where all the sick are congregated, would not be the place of safety; yet there, notwithstanding, it is most likely to be found. Apply this touchstone: it will be seen that the medical officers never suffer more from the disease than their fair proportion, according to numbers; and the more immediate white attendants, orderlies, and others, uniformly less, if the ventilation and discipline be good, than the soldiers in barracks who never go near the place; because, while so employed they are saved from exposure to the sun's heat, to night guards and drunkenness. This I proved from incontestable hospital returns when I was last in the West Indies, as also that the supposed contagion was never communicated to the surgical sick, the convalescent and others, although occupying the most contiguous beds in the same hospital."

With reference to the epidemic which prevailed at George Town, Demerara, from 1839 to 1846, the valuable statistical tables drawn up by Dr. Johnstone, health officer of the port, show that within that period there were received into the colonial hospital 1725 cases of fever (399 of which are returned as Yellow Fever, and 1326 as remittent), whilst 9,153 patients were under treatment for other diseases in the same establishment; yet two only of the medical attendants were seized with fever (one of whom, had just arrived from Europe and the other had been much in Water-street and on the river), and three of the patients, whose attacks can also be satisfactorily accounted for otherwise than by contagion: "they all came from the seat of the epidemic, and brought the seeds of the disease along with them."

Again, in the seamen's hospital, during the same period, there were admitted 3,762 cases of fever, (2,544 of which are returned as Yellow, and 1,218 as Remittent), with 3,422 patients for other diseases; yet but 25 of the seamen previously in hospital (all coming originally from the focus of infection) were attacked with the epidemic,

whilst it can be shown by the records themselves, that these were not cases of contagion.

"It is to be stated," says Dr. Gavin, "that the shipping and shore were the seats of the epidemic; that 3,000 seamen died during the epidemic; and that the seamen had been all more or less exposed to the influence of the poison before admission.

"The fact," says Dr. Blair, "of the opinion of contagion in Yellow Fever being abandoned at Demarara by the unanimous consent of the entire community, professional and non-professional, who had countless opportunities of observing the disease spread over an eight years epidemic, in all its phases, and from every point of view, seems to me testimony and proof as strong as the subject is susceptible of."

Dr. Milroy thus expresses the result of his observation and inquiry respecting Yellow Fever in Jamaica and Havannah;—

"There is one circumstance in the history of Yellow Fever which has made a strong impression upon my mind, as respects its general mode of diffusion, and it is this—The unanimous conviction of the medical men in Jamaica, and also of those in Havannah whom I met, that the disease is not contagious or communicable from the sick to the healthy. I had the opportunity of conversing with the resident medical men in every part of Jamaica, and nowhere did I hear any difference of opinion upon this subject.

"It is certainly a curious circumstance in the history of a disease, that it should be almost universally regarded as not communicable from person to person in those countries where it takes its origin, and is every now and then present either sporadically or epidemically, and where, consequently, the medical men must have constant opportunities of studying its character; while when it occurs in other lands, where it appears only occasionally, at distant intervals, it is denounced as extremely contagious, requiring a rigorous quarantine. Can this strange feature be alleged of any other malady except of Yellow Fever? I do not remember an instance. The circumstance, too, of the comparative exemption of the black and coloured races, who certainly enjoy no immunity from smallpox and other acknowledged contagious diseases, is another fact which, to my mind, is very significant."

It is not, however, surprising that those who for the first time see the sudden spread of a new form of disease should attribute it to contagion, whilst those more experienced, accustomed to observe the same disease under various circumstances, should be convinced that it does

not owe its origin to that cause. Many diseases once universally believed to be communicable from person to person are now believed to be so by none. As long as the real causes of disease were unknown contagion offered a ready explanation.

Humboldt informs us in his Political Essay on the Kingdom of New Spain, vol. iv. p. 171, that persons born and brought up at Vera Cruz are not subject to this disease; and it is the same at Havannah with those who do not quit the country.

“It is incontestable,” he says, “that the Vomito is not contagious at Vera Cruz. In most countries the common people consider many diseases as contagious which are of a very different character, but no popular opinion in Mexico has ever interdicted the stranger not seasoned to the climate from approaching the beds of those attacked by the Vomito. No fact can be cited to render it probable that the immediate contact or breath of the dying person is dangerous to those not seasoned to the climate who may attend on the patient. On the continent of equinoctial America the Yellow Fever is not more contagious than the intermittent fevers of Europe.”

Dr. Fergusson sums up the result of his observation and experience on this part of the subject in the following passage:—

“No experienced men, unblinded by the prejudices of the schools and authorities, or unbiassed by the expectation of Quarantine office, can seriously believe it to be a contagion. It is a terrestrial poison which high atmospheric heat generates amongst the newly arrived, and without that heat it cannot exist; but it affects no one from proximity to the diseased, and cannot be conveyed to any low temperature. This was finely exemplified at Port-au-Prince, St. Domingo, where I spent the earlier months of the year 1796. Our head quarters were the town and its adjunct, Brizzoton, as pestiferous as any in the world, and there we had constant Yellow Fever in all its fury. At the distance of a mile or two, on the ascent up the country, stood our first post, Torgean, where the Yellow Fever appeared to break off into a milder type of Remittent. Higher up was the post of Grenier, where concentrated Remittent was rare, and milder Intermittent, with Dysentery, the prevalent form of disease; and higher still was Fourmier, where Remittent was unknown, Intermittent uncommon, but Phagedenic Ulcers so frequent as to constitute a most formidable type of disease; and higher still were the mountains above L'Arkahaye, of greater

elevation than any of them \* \* \* \* where a British detachment had always enjoyed absolute European health, only it might be called better, because the climate was more agreeable than in the higher latitudes. Here were the separate regions or zones of inter-tropical health, mapped out to our view as distinctly as if it had been done by the draughtsman. Taking Port-au-Prince for the point of departure, the three first could be traversed in the course of a morning's ride. We could pass from the one to the other, and with a thermometer, might have accurately noted the locale of disease, according to the descending scale, without asking a question amongst the troops who held the post; and what kind of contagion must that be which, amongst men in necessary intercommunication, cannot be conveyed from the one to the other? which refuses to mingle with another of lower temperature, although within sight, and so near, topographically speaking, as almost to touch? The men could, and did, constantly exchange duties, but not diseases; and it was just as impossible, and more so, to carry a Yellow Fever up the hill to the post in sight, as it would have been to escape had they been brought down and located amongst the swamps of Port-au-Prince. These things were known to every person in the army, whether medical, civilian or military, and amongst them all there was not to be found a single person who had the smallest belief in contagion, provided always he had been a year in the country, and possessed opportunity of seeing with his own eyes: all, I may say, came out contagionists, myself amongst the number, none remained so. It was impossible that we could, in face of the every-day experience of our lives; and if we had, the very women and drummers of the army would have laughed us to scorn, because they had long discovered there was not the smallest danger to be apprehended from the closest proximity of the sick. The woman knew that when she slept with and attended her dying husband she had not been infected, and all were sensible that no safer duty could be imposed than that of attending the sick bed."

The attacks of Yellow Fever, like those of Cholera, are chiefly in the night. Dr. Smith, states that four-fifths of the patients who came under his observation were attacked at night. He also confirms a statement made by other observers, that in Yellow Fever, as in epidemic fever in general, but particularly in the typhus of European cities, the mortality is commonly less among the weakly and delicate, than the robust and strong

Another resemblance between Yellow Fever, Cholera, and epidemic diseases in general is, that the epidemic poison, whatever it be, affects animals as well as man.

Many writers of eminence on Yellow Fever notice the concurrence of this pestilence with the prevalence of epizootic disease, and in many instances with an extraordinary predominance of insect life; the epidemic poison apparently acting on animals in some such manner as an analogous morbid influence produces blight in plants.

During the Gibraltar epidemic, an unusual mortality was observed among dogs, cats, monkeys, birds, horses, and domestic animals in general; and many of these died with the characteristic symptoms of the prevailing epidemic. In the Yellow Fever epidemic at Boa Vista in 1845 and 1846, there was an extraordinary mortality among the cattle over the whole island, including cows, horses, mules, donkeys, and goats.

The Spanish physicians have recorded similar observations, and they further state, that over the extent of the locality affected by the morbid atmosphere, birds forsake their usual haunts. "Arejula informs us," says Dr. Gillkrest, "that he announced to the inhabitants of Malaga, that their epidemic was about to leave them, because he saw the sparrows returning to their usual haunts."

Yellow Fever, like Cholera, is subject to no rule of gradual progression or proportional severity, but decimates certain localities, while it spares entirely, or visits but lightly, others in the immediate neighbourhood. From observations on West Indian garrisons, during a period of twenty years, it appears that the mortality from febrile diseases in Tobago is 104·1 per 1000, while at St. Vincent it is only 11·2; at St. Lucia it is 63·1, at Barbados 11·8; in Trinidad 61·6, in Antigua 14·9.

The differences are equally striking in different localities in the same island. Thus in Jamaica, the mean mortality from fever for a period of twenty years has been, at Spanish Town, 141·1 per 1000, while at Fort Augusta it has been only 55·5; at Up Park Camp 121·0, at Stony Hill 70·5; at Montego Bay 150·7, at Maroon Town 15·3; at Port Antonio on the north side 126, and at Port Royal on the south 93·9.

"In all the latter instances," says Dr. A. Browne, "with the exception of Port Antonio and Port Royal, the stations are

within short distances, the communications are frequent between them, and any contagious disease prevailing at the one might be easily transmitted to the other; and Port Antonio was contrasted with Port Royal. because the former is a small port where few ships, unless from England or North America, ever enter; and Port Royal, on the contrary, is more frequently visited by vessels from every quarter than perhaps all the other harbours in Jamaica.

“If we corroborate the preceding proofs of the localization of fever by the facts so clearly established by this excellent statistical Report; viz., That epidemic Fever is rarely if ever absent from all the islands in the Windward command, and all the stations in Jamaica, though it has never afflicted them all in any one year, notwithstanding the communications were not interrupted:—and that the removal of sick or troops from one post affected to another exempt from its influence, has so frequently arrested the epidemic, without endangering others, that removal has become the rule, isolation the exception:—The conclusion that the disease which decimates our troops in the West Indies is in its origin truly endemic, appears inevitable; and further, that whenever it assumes an epidemic form, it is not in any way changed in character, or possessed of new or adventitious qualities of propagating itself, however much it may differ in severity.”

The experience of the Yellow Fever epidemics of Spain gives the same result. Thus Velez Malaga lost by this scourge 3 in 7 of its inhabitants, while Cordova lost only 1 in 130; Vera lost half its population, Grenada only 1 in 180; at Carthagen de Levante 1 in 3 perished, at Ximena 1 in 150; at Malaga nearly 1 in 3 inhabitants died, at Ecija less than one in 10.

The experience of such immense differences in the prevalence and mortality of the same disease naturally suggests the inquiry whether they may not be dependent on some peculiar conditions of the localities themselves.

When a careful examination is made of those districts which are the special seats of Yellow Fever, they are found to present substantially the same conditions which have been proved by uniform experience to be at all times attended with a high rate of mortality and at periods of uncertain but frequent recurrence, to be peculiarly subject to violent outbreaks of epidemic disease. These facts are altogether independent of any opinion or hypothesis as to the origin of such diseases. They admit of simple observation and comparison.

We have shown from the experience of every town in Great Britain, and of every part of every town which suffered most severely during the late Epidemic Cholera, the close and inseparable connexion between the extent and virulence of this pestilence and the presence of certain definite, physical, localizing causes ; such as overcrowding, filth, dampness, foul canals and ditches, want of drains and bad drains, and want of water and foul water. In every country within the Yellow Fever zone, where the condition of towns most subject to the visitation of this scourge, and the particular spots in such towns most constantly the chosen seats of it have been examined, we find the same localizing conditions.

The following statements may be cited as examples of the sanitary condition of Yellow Fever districts.

OVERCROWDING.—Dr. Chervin informs us that in the Spanish towns, which are the ordinary seats of Yellow Fever, the first outbreak of the epidemic is constantly found to take place in the most densely crowded localities, to which spots, he says, the disease is often entirely confined ; and he cites, as examples, the outbreak of the epidemic of 1804 at Cordova ; of 1808 at Xeres ; and of 1818 at Cadiz.

The marine, military, and civil officers of Gibraltar, in 1828, concur in representing that garrison as far too densely crowded for the health of the inhabitants.

Thus Dr. Hennen, Inspector General of Hospitals, in a Report to Sir George Don, says :—

“ I have minutely inspected District No. 24, in company with Mr. Wilson of the Civil Hospital, Mr. Woods, the medical officer attached to that district, and other staff officers, and it is with much regret I have to state to your Excellency, that in almost every step I took in that district I had reason for surprise, not that fever had broken out there, but that it had not extended farther. The pauper population is dense to a degree incredible, except to those who have seen it. In sheds, without ventilation, without drainage, and generally composed of the slightest materials, in tiers of beds as close as in a crowded transport, numerous individuals sleep.”

Dr. Broadfoot, one of the members of the Commission of Inquiry, says :—

“ The number of inhabitants in the town of Gibraltar would

be too great for the space occupied by them to ensure permanent health in any climate or country, even with the best planned streets and best constructed dwellings. Many of the largest buildings even, but almost all the smaller ones are greatly overcrowded with inmates, particularly during the night."

Dr. Gillkrest says:—

"Gibraltar is greatly overcrowded. It was not until I had become principal medical officer, in 1833, when I considered it my duty, as general superintendent of the health of the population, to visit all the lanes, alleys, &c., that I could have formed any idea of the densely-crowded and badly-ventilated houses of these localities, and of the difficulty of conceiving how such places should in general prove healthy, amongst a mixed population of about 15,000, exclusive of military, within the walls."

Large masses of the population are necessarily crowded into small and ill-ventilated spaces by a peculiarity in the structure of the streets and courts of Gibraltar.

"The streets," says Dr Broadfoot, "are too narrow and almost totally without those numerous breaks which are requisite for ventilation. The majority of the dwellings, even the best of them, are either built in hollow squares or surrounded by solid walls of from 3 to 8 feet high, as if purposely contrived to prevent any circulation of air. \* \* \* \*

The poorest part of the population live in dwellings of the above description, in the different bends and gullies of the rock, above the better parts of the town, and consequently exposed to the ascending heated air from the habitations below, as well as to the absolute stagnation which it seems does take place in these bends of the rock, frequently during summer."

"A common feature of the dwellings," observes Dr. Gillkrest, "is that of small '*patios*,' or square confined courts, entered by narrow alleys, in which the houses consist of several floors, each occupied by many families."

In a despatch from the Secretary of State to the Governor of Gibraltar (28 July, 1829), in commenting on the proceedings of the Commission, Sir George Murray, observes:—

"With respect to the state of the population, I should not have thought it necessary to repeat my sentiments upon that subject if I were satisfied that Gibraltar is not still overcrowded; but I feel assured that the population continues to

be excessive. Upon this point I refer you to the evidence of Mr. White, the Collector of Revenue, who states that he found 100 persons in a tenement which, as noted on the door plate, should have contained no more than 20; a clear proof of the extreme negligence of the police."

It is not generally known that, according to the army statistical reporters, "the extent of barrack accommodation did not, prior to the year 1827, exceed from 22 to 23 inches in breadth to each soldier serving in the West Indies; and as that would not admit of their having bedsteads, they slept in hammocks, by which expedient as many men could be placed in one apartment as the breadth of their bodies would admit." The amount of space to each man did not much exceed 200 cubic feet. Can we then wonder at the frightful mortality which in those days was continually occurring among our troops?

Even at the present time the space allowed is frequently much too small for safety. The space allotted to the men in the garrison of St. Ann's, Barbados, reports Dr. Gavin, is in general insufficient; for example, in the Stone Barracks, badly constructed, placed in a low situation, and protected from the breeze by a plateau so as, in an extreme degree, to prevent aeration, only from 539 to 543 cubic feet are allotted in the respective floors, while in the lower floor of the Iron Barracks there are at present only 506 cubic feet to each man,—scarcely more than one half the breathing space required to maintain the health and vigour of the soldier in a warm climate.

"Six, eight, or ten persons," says Dr. Milroy, speaking of the ordinary population of Jamaica, "are often crowded at night into a room of eight or nine feet square. The consequences when an epidemic occurs, were frightfully exemplified during the late visitation of Cholera. At Port Maria between one-half and two-thirds of the inhabitants died of this pestilence in the course of three weeks, and an eighteenth part of the entire population was swept away within twenty-four hours: this was in a town population, and we are informed that even this mortality was "very much below what took place on certain estates."

**FILTH.**—Dr. Hennen represents the state of filth of a considerable portion of the Gibraltar population, particularly of the beggars and paupers, as too disgusting to be

described ; and Sir George Murray comments on the "deplorable filth" of the houses occupied by these classes.

"As far as the streets and exterior of the houses are concerned," observes Dr. Smith, "Gibraltar is clean and beautiful, and without dirt ; but when we examine the interior of the houses, a scene of a very different nature presents itself: filth is apparent at every step ; the houses of the poorer classes are extremely dirty."

Two successive Yellow Fever epidemics, namely, the epidemic of 1804, and that of 1813, broke out on the same spot, the dirtiest in Gibraltar, *Boyd's Buildings* ; and the epidemic of the succeeding year (1814) commenced at *Cavallero's Buildings*, "a place which competed with Boyd's Buildings in its state of filth." "Whenever," says Mr. Amiel, "the epidemic has appeared in Gibraltar, it has always commenced in the filthiest spot, and this was the case in the late visitation."

With reference to the Garrison of St. Ann's, Barbados, Dr. Linton reports, that though to the casual observer the quarters of the troops have the appearance of general cleanliness, yet there are many localities in which there are large collections of filth on the surface ; that the medical officers complain that the air is frequently so offensive, more especially in the evenings, that they make it a point to keep their windows closed ; that the soldiers' privy is so great a nuisance that it poisons the whole neighbourhood with its exhalations, the soil being removed every night in boxes and thrown into the sea ; that four hours are usually spent in this occupation, and the nuisance during the operation is intolerable to all in the vicinity ; that the urine from the barracks is carried off by an underground drain which empties itself near, not into, the sea, lodging its contents on the surface, and being always very offensive ; while there is no underground drain at all for depositing the contents of the officers' *pots de chambre* and consequently their servants cannot be prevented from emptying them wherever they can find place and opportunity.

"Every town and village in Jamaica," reports Dr. Milroy, "teems with nuisances of the very worst description ; indeed there is scarcely a house without some accumulation of noxious matter about it. On the general cleansing of the streets and dwellings, and the removal of filth wherever it was found, which took place

on the threatened approach of Cholera, the remark has been made to me everywhere that no one had had any conception of the amount of abominations of all sorts which had been allowed to accumulate around them for a multitude of years."

So at George Town, Demerara, during the epidemic from 1839 to 1846 :

"The river Demerary," says Dr. Johnstone, "and river's edge, or Water-street, was the focus and abiding place of the disease. It there abounded with fearful severity, and with scarcely an exception, every case that occurred elsewhere, could be traced to that impure locality. The cause of this impurity and consequent fixation of the epidemic influence was very apparent in the accumulation of putrescent filth and mud beneath the stellings, or long projecting wharves, which are thickly planted along the river's edge, for the reception of goods. Moreover, many of the buildings in this crowded commercial mart called Water-street, are built on piles driven into the mud and nearly down to the low-water mark; and these piles serve most admirably for the detention of the filth and offal from a daily vegetable market and slaughter-house. That this latter can be no trifling matter it may be observed, that here are slaughtered every morning, cattle, sheep, pigs, goats, &c., sufficient to supply a city with 30,000 inhabitants, besides the surrounding country."

Dr. Gavin confirms the correctness of this description by his own inspection of the locality.

"Nothing," he says, "can to my mind be more demonstrative of the paramount influence of certain localizing causes in developing Yellow Fever than the facts in relation to the late and present epidemics in George Town, Demerara. I had no more difficulty in fixing on the particular spots in George Town where Epidemic Fever would and did first develope itself on its return in 1851-2, and where the force of the epidemic, fitting pabulum being afforded, would expend itself, than I had in indicating the localities in certain districts in Bethnal Green where Cholera would and did prove most prevalent and mortal.

**WANT OF DRAINS AND BAD DRAINS.**—Dr. McLean states that a committee of fifteen physicians which assembled at Barcelona during the prevalence of the severe Yellow Fever Epidemic of 1821, assign as one of the chief localizing causes of the pestilence, the state of the drains and sewers in the town and harbour.

"From the neglect," they say, "of the public police, for many years previously to 1821, at Barcelona, the sewers, drains, canals, and other channels for carrying off the impurities of the city, have

been choked up or become foul to such a degree, that towards the end of June it was impossible to pass by the sea-wall, where they are discharged into the harbour, without suffering from the stench of accumulated and putrefying animal and vegetable substances."

All classes of witnesses concur in stating, that previously to the outbreak of the epidemic of 1828 at Gibraltar, the drains, both private and public, were in a most defective and offensive condition.

"On referring," says Mr. Amiel, "to an official statement of Mr. Woodward's, Surveyor of the Revenue Works, I find that the drains in the lower part of the town have but little declivity, and receive the soil and other filth from those of the upper part, into which the privies of the several houses discharge their contents. The consequence is that, either by direct winds from the west, or eddy winds from the east, the foul air is blown up from the line wall, and in hot weather the streets and houses are filled with air so offensive and foetid as frequently to make the people sick.

"The whole surface of the drains is covered with night-soil, which, from the want of water to carry it off, becomes in the warm weather an expanded ridge of putrid matter near the surface of the ground; and the offensive effluvia, disseminated over the whole place, cannot have escaped even superficial observation. During the late epidemic the air was particularly offensive, and great numbers of rats were found dead in the drains. At their outlets on the line-wall, and in many places where I had occasion to direct their opening, I always noticed that they contained a great deal of filth, particularly at the lower part of the town.

"The drains belonging to the barracks, often completely choked, allow the corrupt substances to accumulate at the entrances, and emit, during the summer months, exhalations highly offensive, which in several instances have been complained of by the men, and reported to the authorities. The drains about the regimental hospital open in the kitchen and in the centre of a small yard, forming a permanent source of poisonous exhalations. The first hospital servant taken ill during the late epidemic was the cook of the establishment, who slept in that kitchen, and the disease has been very severe amongst those who have been successively employed in the same duty.

"Malaria, produced by exhalations from common sewers, probably from its being more particularly the result of animal decomposition, appears to exert a most powerful influence over the character of this fever: and the truth of this fact I have had frequent occasion to observe while I had the medical charge of the Dockyard Department. In 1813 the first patient that I saw with the black vomit was in a gully at the south end of the South Pavilion, in the close vicinity of a drain, the noxiousness of which

had been repeatedly reported to the authorities ; and I was under the necessity of sending to the Civil Hospital, at different periods, from the same neighbourhood, three gardeners, two of whom rapidly died of fever in that establishment. From 1819 to 1822, I repeatedly had under treatment at the Naval Artificers' quarters at Rosia, in the latter part of the summer, cases of fever attended by irritability of the stomach, yellowness of the skin, and other severe symptoms, some of which terminated fatally ; and having observed that a common sewer in the centre of that building was then in a defective state, and occasionally emitted very offensive effluvia, I reported the circumstances to the Commissioners of the Royal Navy in this garrison, as mentioned in my Annual Report of 1823. The drain was removed, and from that period I have not seen or heard of any similar attack of fever occurring in the establishment. During the late epidemic the first cases of fever in the South appear to have occurred in the neighbourhood of the gully near the bridge leading to the hospital, where the offensive vapours of a large drain were very remarkable during the summer months : and the disease has been especially severe in that spot, as in every other place deprived of a pure and free ventilation."

Two of the Commissioners of Inquiry, Dr. Broadfoot and Mr. Howell, confirm these statements. Dr. Broadfoot says—

"The drains were allowed to receive the sweepings of the streets and other solid impurities without being supplied with a sufficiency of water at the upper parts of them to wash away their contents."

Mr. Howell, says,—

"That the drains emitted most foul exhalations is proved by evidence perfectly incontestible. I refer to the testimony given not merely by the medical gentlemen, but by Mr. Woodward, Surveyor of Revenue Buildings, by Lieut.-Colonel Bayley, by Major Middleton, by Colonel Pearson, by Captain Crawford, by Quarter-Master O'Grady, and by others ; that these exhalations were such as not only to pervade most houses, but universally to corrupt the atmosphere, is proved by Major-General Pilkington, the Commanding Royal Engineer."

It has been already stated that the stations where the disease was most concentrated and the soldiers suffered more severely than in any other localities, were the posts on the line or in the immediate neighbourhood of drains.

In the despatch previously referred to, Sir George Murray calls pointed attention to the condition of the general drainage of the garrison.

"I am desirous," he says, "of offering some remarks and suggestions relative to the state of the public drains. All parties agree in opinion that the effluvia which are emitted from the drains are highly offensive at all times, but there is difference of opinion on the point whether such effluvia are injurious to the public health; it being maintained by those who assert that the effluvia are [not] obnoxious, that some of the neighbouring towns of Barbary and Spain, which are unprovided with drains, are in general healthy. I consider this reasoning fallacious, inasmuch as no allowance is made for the action of the air and sun on the exposed impurities of these towns, whereas in Gibraltar, as I shall presently show, those impurities remain concentrated in the drains, and if the late epidemic should owe in any degree its rise to malaria, there is abundant testimony to show that in the month of August last the drains were in a state to produce malaria in its worst condition.

"Mr. Woodward, for instance, states that the drains in Southport-street, which are of great extent, and communicate through the Town Range Barracks up to the spot where the first case of the Epidemic Fever is reported to have occurred, had only been opened once in 14 or 15 years; and Quarter-Master O'Grady, of the 12th Regiment, after alluding to the continual complaints made by the soldiers of the offensive smells emitted from those drains, states that when they were opened after the fever, it had required 20 or 30 carts to carry away the mass of filth with which those drains were choked.

"It may easily be conceived, therefore, that the showers of rain which fell towards the end of August (which the old inhabitants considered as the foreboding of a sickly season, and which Dr. Hennen noted in his Journal in the same sense) would only have the effect of bringing the matter collected in the drains into a state of fermentation, without clearing them of their contents; and as has been stated by Major-General Pilkington, who had taken pains to observe the meteorological changes in the atmosphere, the *westerly winds* which prevailed in July, in part of August, and September of last year, blowing into the orifices of the drains, produced the most deleterious effluvia, and infected every house in the place."

The President of the Commission, Dr. Pym, attached so little importance to this evidence which made so deep an impression on the minds of others that he states, in his Report to the Secretary of State, his conviction that "neither the private nor the public drains of the garrison had any share in generating the late disease;" that, notwithstanding, "since it appears from the most respectable evidence that they were in some

“places excessively offensive,” it might be desirable to “cut off their communication with water-closets, &c, “substituting the use of moveable receptacles of night-soil, as is done in Paris and other large cities;” that the population, instead of being particularly crowded, was “supposed to be less in the year 1828 than in 1826–7;” and that the salubrity of the garrison was positively maintained by the “*westerly winds* which in this climate are “considered the most healthy, and which were more prevalent than usual;” those very winds which Sir George Murray, on a review of the whole of the evidence, declares “blew into the orifices of the drains the most deleterious “effluvia, and infected every house in the place.”

Dr. Chervin states that for some time before the outbreak of the Yellow Fever epidemic of 1818 at Cadiz, “the putrid vapours emitted by the sewers which receive the filth and night-soil of a large portion of the population were so offensive as to exceed all powers of endurance;” and a physician, who visited the city in 1805, Dr. Felix Pascalis, observes:—

“Sometimes in the greatest heat of summer the east, or Levant wind blows fifteen or twenty days without intermission, and the pestiferous gases from the whole filth of the city are continually emitted through the air-holes of the sewers. Is there then,” he asks, “any need for an imported contagion to explain the generation and spread of Yellow Fever?”

On an inspection of the drains of the Garrison of St. Ann’s, Barbados, Dr. Gavin describes them as being “for the most part loaded with deposit, and forming, in fact, elongated soil-pits;” and he calls attention to the sum at present paid for the removal of the filthy boxes of ordure; namely, 75*l.* per annum, equivalent to a capital of 1,500*l.*, for a small portion of which proper water-closets and urinals might be constructed.\*

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\* It has been calculated that every British soldier dying on a foreign station has previously cost the Government from 50*l.* to 100*l.* The total extraordinary civil expenditure consequent on the epidemic of 1828 in Gibraltar amounted to 18,500*l.*: add to this sum the cost of 507 men at the lowest estimate, and we have a loss to the country by a single epidemic at one station, from causes in a great measure, if not altogether preventible, of no less than 43,850*l.* And this in addition to subsequent charges on account of pensions, and passage money for widows and orphans to their native parishes.

In his Quarterly Report ending the 31st March, 1848, Staff-surgeon Spence mentions an occasion in which he himself and several others suffered from the emanations given off by a foul drain.

"Not far from the pavilion, where I am quartered," he says, "there is a drain which used to be foul at all times, but which was not disagreeable in the quarter, because the wind seldom blows from the precise point, south of east, which places us to leeward; but one day towards the end of January it did so, and so much did it oppress me individually that I observed to some one, 'if ever bad smells should cause us to be sick, it would be now,' and about 28 hours after I had a rigor, succeeded by fever; and so in turn had every officer resident in the pavilion."

**DAMPNESS, SWAMPS, AND MARSHES:—**The influence of dampness in promoting the spread of Cholera is universally admitted; the evidence is as complete that it is equally powerful in localizing Yellow Fever, particularly the dampness arising from the foul shores of the sea; the moist slime and mud on the banks of rivers, canals, pools, ditches, and the moats of garrisons; the mire and mud in the unpaved streets, lanes, alleys, and courts of towns and cities; and the ooziings and percolations from sewers and privies, which are often the cause of the humidity of cellar dwellings.

Dr. Chervin states that the main causes of the dreadful epidemics which have ravaged Barcelona are the filthiness of its port and harbour, and the sluggishness and pollution of the canal "Condal," which receives enormous masses of corrupt matter from manufactories, slaughter-houses, wash-houses, and other establishments situated on its banks, the latter forming a pestiferous accumulation of sand, mud, and filth.

Dr. M'Lean states that the Committee of fifteen physicians of Barcelona, in 1821, concur with Dr. Chervin in regarding this canal as a great and constant predisposing cause of epidemic disease.

"Notwithstanding the work which was executed some years ago in the bed of the Condal," they say "the shallowness of the water, the slowness of its course, the constant action of the ardent sun throughout the day; all these causes produced a stagnation of the impurities of the city, and rendered their discharge difficult, giving occasion to deleterious emanations from all points of the canal.

“ The careful examination of the Committee charged with cleansing the port has shown that this water-course was obstructed at its mouth by a bank of sand which, hindering its discharge, had occasioned a considerable collection of stinking water, the product of various manufactures, slaughter-houses, wash-houses, and other establishments situated on its banks, exhaling an insufferable stench.

“ The same Committee found that the foul water, stagnant round this sand bank, was one foot higher than the level of the sea, and more or less in other places.

“ The modern works of the port have converted it into a sort of stagnant pool of which the cleansing has been neglected for several years, producing a focus of infection which had not previously existed.”

There is a universal concurrence among the medical officers who are acquainted with the station, that one of the chief causes of the peculiar liability of the garrison of St. Ann's Barbados to Yellow Fever, is its low and marshy situation, which, according to Dr. Davy, Inspector-General of Hospitals:—

“ Contains, and has in its immediate vicinity, more swamps and lands liable to be flooded, and take on the temporary nature of swamps, than the whole of the rest of the island together, and therefore it may be truly said that, if it does not originate, it must be, to say the least, well calculated to foster the spread of zymotic diseases in every form.”

These swamps are situated at distances from the garrison, varying from two miles and a half, to its immediate neighbourhood. The nearest to it appears to be a tidal swamp, receiving the pernicious mixture of salt and fresh water. There is no question as to the highly injurious nature of this admixture, but some observers regard it as so extremely noxious, that they attribute to this cause the peculiar liability of sea-port towns to epidemics in general, and to Yellow Fever in particular.

“ The Yellow Fever,” observes Dr. A. Browne, “ has so frequently appeared in seaports, as to give a certain air of probability to the assertions of those who believe that this disease must be always imported. Contagionists, however, as well as their opponents, have almost entirely overlooked the fact that such ports, generally situated near the embouchures of rivers, or on narrow inlets having rivers running into them, are more exposed than other places, to the causes of insalubrity, which depend on the mixture of fresh with salt water.

“Now this mixture of fresh with salt water is one of the conditions which is most favourable for the production of malaria,—probably in part from the destruction of organic life which it occasions, and in part from the elements which it furnishes for chemical reactions. The following illustration, taken from the report of M. Melier, will decide this point.

“The locality of the experiment, for such it may be truly called, was the Maremma of Lucca on the shore of the Mediterranean. The ground is divided into three principal basins, interspersed with numerous lakes and pools, which for ages had been inundated from time to time by the sea, thus causing the mixture of fresh with salt water. The insalubrity of this district, we are told, was so great, that ‘inevitable death’ was the consequence of passing a single night during the months of August and September, in this pestilential locality. In order to remedy this sad state of affairs, in 1740, a valved sluice was placed in the Burlamanca,—the channel by which the sea-water entered the principal basin,—so constructed as to shut out the tides when they rose, and to allow the fresh water to escape when they ebbed. These works were finished in 1741. The success was so complete, that fevers which had never before failed to appear, ceased in the following year, and from that time the district became healthy. The village of Viareggio, previously abandoned, became an important place, and a favourite summer residence of the first families of Lucca. In 1768 and 1769, the fevers suddenly reappeared, as in their worst periods. ‘What had happened? Only this: the sluice had got out of repair, and the waters were again mixed.’ The sluice was repaired, and fever disappeared. The deaths which had risen to 1 in 15 were reduced in the following year to 1 in 40 of the inhabitants. The same accident occurred in 1784-5, with similar consequences, and the evil was remedied in the same manner.

“Similar sluices were established at Cinquala in 1812, at Motrona in 1819, and at Tonfalo in 1821. Everywhere the success was the same; the experiment is as conclusive as it is undeniable. Similar results have since been experienced at other places in Italy, and in France.

“The pond of Lindre-Basse, in the department of the Meurthe, affords a curious illustration of the effects of the different conditions under which malaria is generated, in modifying diseases arising from paludal infection. This fish-pond, managed according to the triennial rotation system common in Sologne, is two years under water, and one year dry. In the first year it is half filled, and gives rise to intermittent fevers; in the second year it is full, and typhoid fevers prevail; in the third year, after being fished, it is left dry and cultivated as a field, and in this year carbuncular affections appear. These diseases have succeeded one another as regularly and invariably as the dif-

ferent states of the pond, for a period of 16 years;\* and the idea naturally suggests itself, that diseases which have a common origin, must have more or less a common nature, however much they may differ in outward appearance."

In tracing the influence of paludal exhalations on the troops in Flanders during the campaign of 1748, Sir John Pringle states,—

"The troops had scarcely been a month in the cantonments when the return of the sick amounted to 2,000, those who were near the marshes suffering by far the most, both in the number and the violence of the symptoms."

He relates,—

"That the Greys cantoned at Vucht (a village within a league of Bois-le-Duc, surrounded with meadows either then under water or but lately drained) were the most sickly; that for the first fortnight they had no sick, but after continuing five weeks in that situation, they returned about 150; after two months, 260, which was about half the regiment; and at the end of the campaign they had in all but thirty men who had never been ill. That a regiment at Nieuland, where the meadows had been floated all winter, and were but just drained, returned sometimes above half their number; that the Scotch Fusileers at Diuther, though lying at a greater distance from the inundations, yet being quartered in a low and moist village, had above 300 ill at one time; while a regiment of Dragoons cantoned only half a league south-west of Vucht, were in a good measure exempted from the distress of their neighbours. Such was the advantage even of that distance from the marshes, of the wind blowing mostly from the dry grounds, and of a situation upon an open heath somewhat higher than the rest."

Observations have been made which seem to show that the noxious influence of marsh exhalations is confined within somewhat narrow limits, or, at all events, that removal to short distances is often a protection from them.

"The distance," says Dr. Bancroft, "to which the exhalations of marshy grounds may be conveyed from their source, and retain the power of causing the yellow and other marsh fevers, will partly depend on the force of the wind, and partly on the extent of the surface from which they arise, and on their being

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\* "A change in the rotation in 1848-9 has altered the order of succession of these diseases.—*Comptes Rendus*, 1850."

more or less copiously extricated from that surface. If the wind be very moderate, and blow steadily from the same point, and if the miasmata be abundantly emitted from a very great extent of surface, it seems probable that so large a mass of them as would thus be formed might be conveyed a quarter, and perhaps half a mile, before it became so diluted with atmospheric air, or so dissipated by the wind, as to lose its morbid power.

“Lemprière says, that while the most disastrous consequences resulted from close proximity to the land, the crews of vessels stationed in the very narrow channel, less than a quarter of a mile between Beveland and Walcheren, continued healthy during the whole campaign.”

“The La Ventille hills,” says Dr. Gavin, “bound Port of Spain, Trinidad on the east and south. Those whites, and even coloured persons, who attempted to live on the southern side of the hills invariably die after a short time of fever, from the emanations of the wide spread pestilential ‘Caroni’ swamp which for some distance skirts the base of the hills ; and a military post erected near the declivity leading to the swamp had to be abandoned as utterly untenable. The magazine lower down is also so unhealthy as to require a constant and regular change of sentries. Nevertheless, on the other side of the hills, which are covered with wood, white persons, and even newly-arrived Europeans, reside with comparative immunity—at least from severe forms of fever.”

“Dr. Blane records that at Rock Fort, Jamaica, the distance of two cables’ length was sufficient to make the difference between great sickness and complete immunity from disease.”

As a somewhat extreme case, it is stated that the marsh exhalations from Brouage, when carried by the south-west winds, affect the inhabitants of Rochefort, a distance of four or five miles.

These statements appear to be a correct expression of the general fact, but there is nevertheless unquestionable evidence that in certain states of the atmosphere, and by currents of wind blowing steadily in a particular direction, these emanations may be wafted to much greater distances than four or five miles. As for instance, in British Guiana, where, when the land wind blows over the hundreds of miles of swamps in the interior, the inhabitants invariably become affected with sudden and severe attacks of fever. It has been long observed that when the atmosphere is so damp as to form visible vapour or fog, these pernicious emanations are carried not only in greater

abundance but to greater distances than when the atmosphere is dry.

Sir John Pringle gives the following illustration of the intensity with which such exhalations may be accumulated in fog, and conveyed in the highest state of concentration.

“When the troops were in Zealand,” he says, “they had not been a fortnight in the cantonments before several of the men belonging to those regiments which were stationed nearest the marshes were seized simultaneously with lassitude and inquietude, a sensation of burning heat, intense thirst, frequent nausea, sickness, and vomiting, aching of the bones, pain in the back and violent headache. There were some instances of the head being so suddenly and violently affected that without any previous complaint the men ran about in a wild manner and were believed to be mad, till the solution of the fit by a sweat and its periodic return discovered the true nature of their delirium. Most of the men were first taken ill upon their return from forage. The regiment being cantoned close upon the inundations, and many of the quarters being above two leagues from the place where the magazines were kept, the men were obliged to set out about four in the morning, in order to get back before the greatest heat of the day. At this early hour the meadows and marshes on each side of the road were covered with a thick fog of an offensive smell. The party generally returned before noon, but several of the men, even before they could get back to their quarters, were already in a violent fever; some in this short space of time were actually delirious; and a few on their way home were so suddenly taken with a frenzy as to throw themselves from their trusses into the water, imagining they were to swim to their quarters. One man on reaching home, was suddenly seized with intense headache, got out of his quarters, and ran about the fields like one distracted. Three years after this sickness it was found that two of the men who were thus suddenly affected with frenzy, though they recovered of their fever, had ever since been epileptic, and that all the rest who had been ill remained exceedingly liable to returns of an intermitting fever.”

Sir John Pringle observes, and the statement has been confirmed by subsequent experience, that a moderate elevation in a swampy district is a more dangerous position than a lower one. St. Ann's Fort, Barbados, is only about fifty feet above the level of the proximate tidal swamp, and offers, from its physical conformation, no material barrier to a sweep of air from the marsh over the

fort; and all the medical officers serving on this station are fully aware of the influence of this swamp in modifying and aggravating the epidemics with which the garrison is so often visited.

"The huts in Jamaica," reports Dr. Milroy, are "placed on the bare ground; there is never even a gutter provided to carry off the rain, which therefore soaks into the earth all around the wattled walls, not a little finding its way into the interior, independently of what penetrates the roof, which, if thatched, as it generally is, becomes like a wet sponge."

On a review of the histories handed down of such epidemics as have been the subject of a particular narrative, it is found that they all contain statements of facts which illustrate in the most impressive manner the influence of the localizing causes now enumerated. This is especially the case with the Gibraltar epidemics of 1804, 1813, 1814, and 1828, with respect to each of which it appears that the spots in which the disease commenced and committed the greatest ravages were the most remarkable for their over-crowding, filth, dampness, and inefficient or pernicious drainage. So with respect to the epidemics of St. Ann's in 1805, 1816, 1847, 1848, and 1849, the experience is the same. Of the epidemic of 1805, for example, it is stated that the particular barrack in which the disease chiefly prevailed was crowded to excess; that it was impracticable to ventilate the apartments on the ground floor; that this ground floor was at the same time extremely damp, and that the men on this floor were attacked in the proportion of three to one of those in the upper story.

In the epidemic of 1816 the spot named in the record of the day as the "*fons mali*," is described as having been in an extreme state of filth from collections of ordure, mud, refuse of swine, and other putrefying matters, giving off the most offensive smells. Here, in a row of buildings called the "Huts," consisting of a long shed partitioned off into separate compartments, each compartment containing barely 1,000 cubic feet of space, yet lodging six persons, and consequently each

person having only 166 cubic feet of space to breathe in, the pestilence first broke out, and continued through its whole course to number the largest proportion of victims.

Dr. John Davy and Dr. Collings ascribe the epidemics of 1847, 1848, and 1849 to an accumulation of rank vegetation with urinous exhalations from a neglected hollow between the Artillery and Stone Barracks; the defective drainage of the Savannah, and adjoining ground; the state of the grave yard; the breaking up of a considerable extent of ground to some depth,\* and levelling it for the improvement of the works in the neglected barracks where the disease first appeared, together with the imperfect ventilation, and the overcrowding of the quarters of the men. On this occasion the disease appears to have been confined to the garrison during the whole period, and for a considerable portion of that period, to the barracks and quarters where the drainage was the most defective, and the localizing causes enumerated the most active.

It is not often that an epidemic is capable of being traced to its local source with so much precision as in the case of the outbreak of Yellow Fever in the army of the United States at Gallipolis.

“The fever,” says Major Prior, “was, I think, justly charged to a large pond near the cantonment. An attempt had been made two or three years before to fill it up, by felling a number of large trees that grew on and near its margin, and by covering the wood thus fallen with earth. This intention had not been fulfilled. In August the weather was extremely hot and uncommonly dry; the water had evaporated considerably, leaving a great quantity of muddy water, with a thick slimy mixture of putrefying vegetables which emitted a stench almost intolerable. The inhabitants of the village, principally French and very poor, as well as filthy in their mode of living, began to suffer first, and died so rapidly that a general consternation seized the whole settlement. The garrison continued healthy for some days, and we began to console ourselves with the hope that we should escape altogether: we were, however, soon undeceived, and the reason of our exemption heretofore was soon discovered. The wind had blown the air arising from the pond from the

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\* This breaking up of ground seems to have been a frequent cause of fever among the French troops in Algeria, and is noticed by nearly all the late writers on the subject.

camp; but as soon as it shifted to the reverse point, the soldiers began to sicken: in five days half the garrison was on the sick-list, and in ten, half of them were dead. They were generally seized with a chill, followed by headache, pains in the back and limbs, red eyes, constant sickness at stomach or vomiting, and generally just before death with a vomiting of matter like coffee-grounds. They were often yellow before, but almost always after death. The sick died generally on the seventh, ninth, and eleventh days, though sometimes on the fifth and on the third. As some decisive measures became necessary to save the remainder of the troops, I first thought of changing my quarters, but as the station was in every respect more eligible than any other, and had been made so by much labour and expense, I determined to try the experiment of changing the condition of the pond from which the disease was believed to have arisen. A ditch was accordingly cut; what little water remained was conveyed off, and the whole surface covered with fresh earth. The effects of this scheme were soon obvious. Not a man was seized with the worst form of the fever after the work was finished. And the sick were not a little benefited—for they generally recovered, though slowly, because the fever became a common remittent, or gradually assumed the intermitting form. A few cases of remitting and intermitting fever occurred occasionally till frost put an end to it in every form. As soon as the contents of the pond were changed by cutting the ditch, the cause, whatever it was, seems to have been rendered incapable of communicating the disease in its worst form.

Professor Yandall gives an instance of the influence of the dampness connected with ponds of stagnant water in causing the frequent recurrence of malignant forms of fever.

“Louisville, in Kentucky,” he says, “is situated on the south bank of the Ohio river, at the Falls, on a beautiful plain, 70 feet above low-water mark, stretching back and gently declining from the river.” The rock of which the subsoil is composed “forms a surface remarkable for its evenness, and the soil which it produces, as it crumbles under the action of the air, frost, and water, is peculiarly retentive of moisture. Ponds and slushes are abundant, wherever the black slate constitutes the surface-rock. The first houses erected at the Falls were built in the midst of ponds. Entire squares of the city are now pointed out, which occupy the ancient beds of ponds, large and deep enough to float a steam-boat. These have all been drained, and such collections of water are nowhere to be seen within the city limits.

“Louisville, while it stood amid its ponds, was regarded as one of the most sickly towns in the valley of the Mississippi. It was

commonly called '*the grave-yard of the West.*' It is now esteemed one of the most healthy. Intermittent fever was a regular annual visitant; and occasionally a form of bilious fever prevailed, rivaling yellow in malignity, and threatening to depopulate the town. The most fatal of these endemics broke out in the summer of 1822, after a hot rainy season. The number of victims from it, out of a population less than 5000, was 232. In a family consisting of 20 persons, 19 were sick at one time, and in some families every individual died. At this time only one street in Louisville was paved, and within its limits were at least eight ponds of greater or less dimensions, most of which, in the course of the autumn, were dried up, exposing foul bottoms to the sun."

We cite the following as an example of the extraordinary mortality which may be produced by the concentration of some of the more powerful of these localizing causes in unusual intensity.

In the Bahamas, says Dr. A. Browne, quoting from the Statistical Report—

"We find that the mortality among the white inhabitants of all ages was about three times as high as in Britain, while the mortality among the white troops there was 13 times as great as at home.

"The principal barrack was, till lately, at Fort Charlotte, a spot notorious for its insalubrity. It is situated on the summit of the little ridge of ground in rear of the town of Nassau, and surrounded in every direction, except towards the sea, by extensive marshes, the exhalations from which, during the morning and evening, generally envelope the barrack in a dense fog.

"Shortly after it was erected, in the end of the last century, nearly the whole of the 47th Regiment, including men, women, and children, were swept off by Yellow Fever within a few weeks. In 1802 the 7th Fusiliers buried 220 out of 300 within as short a period, and such was the virulence of the disease, that out of 12 officers attacked, one only recovered. In the following year it again broke out, and reduced the remainder of this force to 50 men, whose lives were for a time saved by removal to a neighbouring island, where only one died in the course of three months; but immediately on their return, the commanding officer and almost every man of this ill-fated body fell victims to the insalubrity of the fort. For some years afterwards no European troops seem to have inhabited it; but 70 men of the 58th Regiment were sent there in 1818, who lost about 40 in six months, besides 13 out of 37 women and children; not a man of the whole force was left fit for duty; and the lives of the survivors were only saved by removal to a small

island about a mile and a half distant. The lower rooms of this barrack proved much more fatal to the inmates than the upper, and the hospital appears to have been still more unhealthy in its site; so much so, that the white soldiers, in sickly seasons, looked upon admission into it to be equivalent to a death-warrant. Though the health of the black troops is, in general, but little affected by those febrile diseases which prove so inimical to the white, even that class, when occupying the same barrack suffered severely, particularly the men in the lower rooms. In 1828, out of 80 in these rooms, 17 died, or nearly a fifth of the whole, while out of 210 in the upper rooms, but 8 died; and in the new barracks at Fort Nassau only 3 died out of 180.

“The barrack at Fort Nassau has always been healthy, the accommodation good, and the sickness and mortality among the troops there exceedingly low. \* \* \* These remarks, in regard to the barracks, have been deemed necessary, in order that incorrect inferences may not be drawn as to the climate of the Bahamas, by attributing to it a degree of mortality which seems to have been, in a great measure, owing to the troops having continued, for a long series of years, to occupy a position, which, from its extreme insalubrity, was not unaptly termed the abode of death.”

We might multiply similar instances to an indefinite extent, but the facts already cited suffice to show the connexion between the outbreak and prevalence of Yellow Fever and the presence of localizing causes definite, palpable, often restricted in their operation within narrow limits, and removable.

So far as opportunities of observation have yet been afforded, the same conclusion, which recent experience has amply proved with respect to Cholera, is established with respect to Yellow Fever, that in proportion as these causes are removed or diminished, Yellow Fever and other epidemics cease to appear in the improved localities, or recur at more distant intervals and in milder forms.

Thus from official correspondence it appears that in consequence of the local causes of disease which were brought to light in the garrison of Gibraltar on the occurrence of the Yellow Fever epidemic of 1828, important measures were adopted for the improvement of the drainage and general cleanliness of the garrison, as well as for the prevention of overcrowding, and from that

period up to the present time the garrison has been comparatively exempt from disease.

Similar results have of late years been experienced in the various towns on the Atlantic sea-board of the United States, since the judicious enforcement of regulations for the protection of the public health.

Dr. John Davy bears, in the following words, his testimony to the like beneficial results of improvements effected under his own observation in the garrison of Barbados.

“ Referring to returns of sick of the troops in this continent, I find proof that for a series of years there has been less tendency here to the occurrence of this fatal fever (Yellow Fever), accompanied as it were in relation of cause and effect, with improvement in the ground as to drainage in the neighbourhood of the barracks, and in the provisioning of the men, and the avoiding of crowding them, a coincidence that will be considered, I hope, sufficiently encouraging to tend to further ameliorations with a like prospect of success.”

“ Several well marked instances,” says Dr. Milroy, “ of the abatement or total cessation of fever by draining and clearing the ground in the neighbourhood of dwellings were mentioned to me in different parts of Jamaica. Mr. Bruce, one of the practitioners in the parish of Vere, quoted his own residence as a striking illustration of this fact.”

We submit that the preceding evidence establishes a complete identity between the localising conditions of Yellow Fever as they are found to exist within the Yellow Fever zone, and those of Cholera and other epidemic diseases over all the districts in which they prevail.

Medical authorities appear to be agreed that the conditions which have now been described, and the like, are not in a proper sense the causes of epidemic diseases, but rather the circumstances which determine the actual outbreak, spread, and intensity of such diseases when an epidemic influence is present. What that epidemic influence is, constituting the true material cause of any prevailing epidemic, the present state of medical science is unable to explain. The filthiest localities, the most crowded quarters and dormitories, are not at all times the seats of those particular forms of disease called “ fevers,” though the inhabitants are generally in a depressed state of health. Even in epidemic seasons some such places

often escape. While cities and countries in a bad sanitary state are sometimes devastated by pestilence, at other times, their sanitary state remaining unchanged, these same cities and countries continue free from pestilence for indefinite periods, the intervals occasionally numbering many years; and then sometimes suddenly, but more frequently by slow and appreciable degrees, pestilence again returns and rages with its former fury. Why this is—why conditions which sometimes act so powerfully do not universally and invariably produce epidemic disease, is a problem yet to be solved. What is certain, however, and what is of the utmost practical importance is, that a bad sanitary condition is at all times attended with an excess of sickness and mortality, and that whenever epidemic diseases do prevail, they concentrate their violence in these unhealthy localities. Still the sanitary conditions, as far as is apparent, remaining the same, epidemics return only periodically. It has been generally inferred therefore that for the development of that extraordinary amount of the same kind of disease simultaneously amongst great numbers of the population which constitutes an epidemic, there must be, besides the known conditions, the presence of some other condition as yet unknown; and hence these known conditions are commonly said to be not generating, but localizing causes; the unknown or real causes being, in our ignorance of their true nature, compared to poison clouds which pass from city to city, and from country to country, bearing and scattering poison germs, the seeds of pestilence, for the development of which the localizing conditions that have been described afford the fitting and perhaps necessary nidus.

But there is another question of great importance on which medical opinion appears to be less unanimous; namely, the extent to which filth, overcrowding, and decomposing organic matter or the like, may actually generate fever. Though it is generally agreed that these causes cannot of themselves produce such epidemics as those in question, yet the evidence is indubitable that they are capable, when concentrated, of generating individual and even numerous cases of fever of different types, and of the highest degree of malignity. Sir John Pringle and others state, with reference to bodies of men, that when-

ever wounded soldiers, with malignant sores, or mortified limbs, were crowded together, or whenever only a few of such diseased persons were placed in a room with the sick from other diseases, for example, with those labouring under intermittent and remittent fevers, a severe and mortal typhus quickly arose; nay, that whenever men previously in a state of sound health, were overcrowded in rooms without ventilation, typhus fever was sure to be produced.

From indubitable facts it is argued that we see at the present time the actual generation of typhus in a similar manner, constantly going on before our eyes. One of the physicians of the London Fever Hospital, gives the following illustration of the fact:—

“A number of Irish arrive in London, and crowd together in the close, undrained and filthy courts in Grays Inn Lane. They are healthy when they arrive from the country: there was no fever in the neighbourhood when they came. Twenty or thirty persons occupy a room containing barely space enough for the healthy existence of four. All sorts of abominations accumulate around these overcrowded dwellings. In a few days typhus breaks out. During the spring of the present year, (1851) upwards of one hundred cases of typhus have been received into the London Fever Hospital from courts occupying one side only of Grays Inn Lane. The disease has not spread to the other side of the street; it has not extended even to houses in the infected courts that are in a cleaner and less crowded state; the prevalence of the fever has been strictly limited to the houses and rooms in which the common localizing causes are very intense.”

That Yellow Fever may be so produced, in the climate of Yellow Fever, is placed by every day occurrences in those regions beyond the possibility of doubt. So much indeed is this the case, that a physician after many years service between the tropics, gives the following receipt for the sure and rapid production of Yellow Fever:—

“Take,” says Dr. Bone, “of soldiers lately arrived in the West Indies any number; place them in barracks in a low wet situation, or in the mouth of a gully, or on the brink of a dry river, or on the summit of a mountain, and to leeward of a swamp, or of uncleared ground, and where there is no water or only bad water; give them each only twenty-two inches of wall in their barrack room; let their barracks be built of boards or lath and plaster, and have neither galleries nor jalousied win-

dows, but close window shutters, and a hole or cellar under the flooring for containing mud and stagnant water, and holes in the roof for the admission of rain, and the windows only eighteen inches from the floor, that they may be obliged to sleep in the draught of air; and let them have drill every morning on wet ground, and when fasting; guard mounting, and all kinds of fatigue not in the morning or evening, but during the hottest time of the day; when on sentry, no shed to keep off the direct rays of the sun; bad bread, putrid meat, few vegetables, plenty of new rum, especially in the morning; discipline enforced by terror and punishment, not by mind and prevention; an hospital similar to the barrack room, without offices, always crowded, plentifully supplied with rum, scantily with water, and so ill regulated that the men dread to enter it; a firm belief in the doctrine of contagion, and a horror of approaching any person affected with Yellow Fever. Let these directions be attended to in Trinidad, or even in Barbados, and especially when the air is stagnant, or charged with noxious vapours subsequent to long drought, the soldiers will soon die, some of them of Yellow, some of them with Black Vomit, and those first, in the rooms where these directions have been most carefully observed."

It is proved by melancholy experience that when due care is not taken to prevent the concurrence of these and similar conditions, individual cases of Yellow Fever, in a Yellow Fever climate, may be as surely produced as cases of typhus among the Irish in the courts of Grays Inn Lane and St. Giles's. Dr. Fergusson gives an instance of this, which may be taken as an example of a general rule.

It appears that the "Regalia" transport sailed with black recruits from the coast of Africa for the West Indies in 1815; it is stated that the ship was good and her crew healthy until she took on board a large quantity of green wood for fuel a short time before the blacks were embarked. Many of the black recruits were embarked sick from hospital with ulcers, fluxes, &c., and they had no surgeon to attend them.

"The quantity of green wood laid in at Sierra Leone for fuel," says Dr. Fergusson, "must have been very considerable, for after she had been several weeks in the West Indies, there were still as many tons of it left as in the master's opinion would serve for a voyage to Europe. The ballast, too, had never been changed or shifted from the time she left England, nor for any

discoverable time before. It was what is called shingle ballast, small stones with a considerable mixture of mud and other impurities; and when I examined it on board the 'Regalia,' it had been much fouled by leakage from the water casks. The ship in respect to leakage, had 32 inches of water in the well at the time I sounded it, and, according to the master's calculation, she made three-quarters of an inch every hour. A ship navigating the tropical seas with a quantity of green wood recently laid in, and foul ballasting that had not been changed for years, and impregnated with the gases arising from putrid sea water, I conceive perfectly adequate to the production of the most destructive fever, by furnishing morbid miasmata, similar to those that on land arise from marshes when exposed to the influence of the higher degrees of atmospheric heat."

In a few days after sailing, Yellow Fever broke out in the "Regalia" with such violence that the men continued to fall ill one after another on the passage until all except one boy had been attacked, and five out of twenty-one died before the arrival of the vessel at Barbados; and on her arrival there almost every one was seized with fever that came on board to supply the place of those that had perished.

We submit that here is an instance of the direct and positive generation of Yellow Fever under circumstances which would be commonly considered as merely furnishing the localizing conditions; and consequently that there are known and definite circumstances which alone suffice for the actual production of fever. But diseases produced in this way, whether typhus in England or Yellow Fever in the West Indies, do not spread beyond, and are strictly confined within the boundary of the localizing conditions, being thus broadly distinguished from the same diseases when they assume an epidemic character. Every carefully observed instance of the local outbreak and spread of fever on record, evidences the universality of this distinction. The sequel of the history of the "Regalia" affords an apt illustration of it. From authentic documents published with the original narrative of this case, (see *Medico-Chirurgical Transactions*, Vol. 8, pp. 114 and 156,) it appears that through an oversight of the inspecting medical officer, the ship on her arrival at Barbados, with the Yellow Fever on board, was not put under restraint or Quarantine; but communicated freely

with the sea-ports of Barbados, the Saintes, Antigua, and Guadaloupe; landing the severally ill or dying subjects of that disease amongst the inhabitants, and at the hospitals at Barbados and Antigua, without communicating infection at any of these places.

A further illustration of the same fact is afforded by the case of the ship "Donostiarra," referred to by Dr. Gillkrest. It appears that this vessel had sailed from the Havannah with a clean bill of health, and after undergoing the usual quarantine of 10 days at Corunna, she touched at St. Andero, and arrived at her destination, the port of Passages, with a healthy crew. Her cargo, consisting chiefly of sugar and tobacco, had been discharged; for several days many people went on board without any disease having broken out either amongst them or the crew. But first a Custom-house officer who had been several days on board, was taken ill, and died on the third day, black vomit having appeared. This man was said to have been much engaged in the hold, looking after contraband goods. Seven days afterwards a man who had been in the hold, surveying the ship's timbers, likewise died. Some of the planks of one of this vessel's sides having been found decayed, 12 carpenters were employed in removing them; six of these men were attacked with fever in quick succession. On the fourth day from the commencement of the opening of the ship, the disease began to appear in an unequivocal form on shore in the houses close to which the ship was moored. But it did not extend beyond the houses opposite the ship. On careful inquiry it appeared that although some persons were attacked whose habitations were at a distance, yet all so attacked had been, and had remained for some time within the space to which the malaria from the ship appears to have been limited. The names and occupations of those persons are given by Dr. Arrutti, the writer of the original account, who states that the heat at this time was excessive (96° Fahr.); that the course of the wind favoured the conveyance of the noxious emanations from the ship to the houses; that he ascertained each house in which persons were attacked, and the several points to which individuals labouring under the disease were conveyed; and affirms, that notwithstanding the

adjuncts of "crowded, filthy, and badly ventilated habitations, the disease did not spread; whether they died or recovered, to none out of the focus was the disease communicated."

In reporting on an instance of the spontaneous production of Yellow Fever on board of Her Majesty's Ship "Pilot," from December 1840 to March 1841, at Carthagena—Deputy Inspector Evans, after noticing the arrival of the vessel at the Dockyard of Port Royal, Jamaica, for the purpose of being cleaned, proceeds as follows :—

"In attempting to perform this duty, stoves, with lighted fires, were put down to air the hold, as a precautionary measure; but every fire and light that was used was immediately extinguished from the foul air that had been generated. About three cartloads, or, according to some, about a punt-load of black mud, was finally removed from the hold, amongst which mud decayed timber in considerable quantity was mixed. This vessel had not been cleared out from the time of her arrival on this station, upwards of two years and some months.

"As the 17 men who were received into hospital after the 22nd January were attacked, when employed in the duty of removing the mud, it is fair to infer that they derived their illness from some cause on board the 'Pilot,' originating from the state of the hold, rather than from the marshes at Carthagena, fever not being prevalent in any other ship at Port Royal at the same time."

Some sporadic cases of fever, remarkable as having appeared at an elevation of upwards of 4,000 feet occurred during the autumn of 1848 in the family of a sergeant at Newcastle barracks, Jamaica. These cases were treated by Dr. McIlree, now surgeon to the 16th Regiment, who speaks of yellowness of skin as one of the symptoms; and on post mortem examination, he found black matter in the stomach of two of the subjects.

"Its origin," says Deputy-Inspector Dr. Watson, "may fairly be traced to the powerfully morbid agency of the exhalations from a cesspool, which was found to exist near the serjeant's dwelling, and perhaps to some collateral causes.

"The cesspool alluded to had been dug about three or four months ago, and contained, besides the ordure which had been deposited in it,—'an immense quantity of filth and soil carried from the privies when cleaned out'—a very large quantity of water. It was situated to the windward of the serjeant's hut, about 30 yards distant, and on the same level, so that the eddies

of the north wind sweeping down the gorge of the valley doubtless threw the whole volume of noxious vapours on the hut ; while they carried the pestiferous exhalation clear over the hut (situated lower down the hill) of the corporal, who, with his family, were quite exempt from any sickness. The serjeant had raised his hut with a coating of mud, six or eight inches thick, and in the neighbourhood a quantity of bush had sprung up since the rains had set in."

Dr. Watson adds—

" It must be kept in mind that it (this case) distinctly and incontestibly proves that a fever of a malignant character and fatal tendency may be engendered by the agency of local causes, even on mountain stations, at an elevation generally supposed to be beyond the fever range ; and hence that it is a matter of policy, as well as of humanity, to adopt with promptitude such sanitary measures as are calculated to secure the troops from the invasion of a disease of a like nature to that which has called forth these remarks."

Such is the uniform tenor of the evidence in relation to these local outbreaks of disease. But if it be a fact, as these and similar narratives appear to place beyond doubt, that diseases thus generated by local causes, are strictly limited to the localities in which they take their origin and do not spread beyond them—if endemic causes indubitably capable of generating sporadic cases, cannot produce an epidemic—it would seem in the highest degree improbable that the mere introduction of sick persons into a healthy community can do so.

For to effect this the sick must not only bring with them disease, but at the same time create and communicate those peculiar conditions, whatever they may be, on which the existence of epidemics depend. That such conditions are not ordinarily produced by the presence among the healthy of one sick person, or by that of a body of sick persons, we know, because we know that commonly the admission of the sick into towns previously healthy, and into hospitals, is followed as has been fully shown, by no communication of disease whatever, much less by the creation of an epidemic. In any alleged instance of this, therefore, such instance being an exception to ordinary experience, it is but reasonable to demand that it should be substantiated by such clearness and weight of evidence as is

required in other cases to establish a fact antecedently improbable.

In our First Report we observed that such authorities as Drs. Haygarth, Percival, Ferrier, Carmichael Smith, Currie, Russell, Roberts, Arnott, Christison, and others, deny that exhalations from the living body are capable of permanent suspension in the atmosphere, or that they can be conveyed unchanged through pure air to great distances. They regard it as established by an indubitable body of evidence, that the moment these exhalations come in contact with the external atmosphere, they are diffused through it: that by such diffusion their injurious properties are destroyed, and that, though when pent up in close unventilated rooms or filthy ships they may acquire some degree of permanence, and much concentration and virulence, yet, when they once pass into the ocean of air, they disappear as a drop of rain in the ocean of water. These authorities view the property thus possessed by air to neutralize and destroy these exhalations as a provision of nature for our well-being.

We further observed that if the emanations thrown off from the living body formed permanent and powerful poisons like the miasma produced by decomposition, and if like such products they were capable of being conveyed unchanged to great distances, we should be able to live only in solitude; we could never meet in society, for we should poison each other; the first symptom of illness would be the signal for the abandonment of the sick, and we should be compelled by a due regard to self-preservation to withhold from persons afflicted with disease every degree of assistance that required personal attendance.

But our physical, is in harmony with our social constitution, and not a contradiction to it. The necessity of intercourse between all the members of the human family is one of the final necessities of our race. The policy of encouraging and facilitating that intercourse among all the nations of the earth is one of the favourable distinctions of our age. The great discoveries in science, the wonderful facilities which have resulted from some of them for personal and commercial transit and correspondence, all

have the effect not only of connecting the remotest cities and towns of individual nations, but of bringing the most distant countries into close neighbourhood and familiar intercommunication with each other. But if it be true that plague and pestilence are capable of being imported from country to country, bringing devastation in their course, and that this calamity may be prevented, and can only be prevented by placing a rigid barrier between one nation and another, so as effectually to obstruct their intercourse, then there is a contradiction between the necessities and obligations of the human family and the physical laws of their being; a contradiction in the highest degree improbable, since we see it in no other part of nature, and which therefore ought not to be admitted as a truth, and much less acted on as a principle, without the clearest and most indubitable proof.

We now proceed to show that there is no such proof; that the alleged communication of epidemic diseases from one country to another rests on no sufficient evidence, and that when the so-called facts adduced in proof of the allegation are closely examined, they are found to be so loose, so inconsistent with admitted events, and so inconclusive, that they would not in a judicial or scientific inquiry be acknowledged as evidence at all.

In the following examination we propose to include all the cases on which any material reliance is placed, not omitting one, as far as we know, which would in the present day be regarded as deserving consideration.

One of the most celebrated cases of alleged importation is that represented by Dr. Chisholm to have taken place in the year 1793. Up to this period Dr. Chisholm admits that Yellow Fever, as it had hitherto existed in the West Indies, had always been derived from marsh miasmata, and was unquestionably destitute of contagion; but he affirms that in that year, a *nova pestis*, a peculiar, original, foreign plague, recently generated, and utterly unknown before, endued with a distinct character, possessing new powers of devastation, and capable of propagating itself throughout the world, was introduced by the ship "Hankey" into Grenada on the 19th of February 1793; that from Grenada

this new pestilence was propagated over not only a great part of the West Indies and North America, but also to Gibraltar, Cadiz, Malaga, Carthagená and other places in Spain, and that in some of these places it supplanted the Yellow Fever properly so called.

Dr. Chisholm's account of this extraordinary event is in substance as follows:—About the beginning of the month of April 1792 the "*Hankey*" sailed from England in company with another ship, the "*Calypso*," both chartered by the Sierra Leone Company, loaded with stores and adventurers for a projected colony at the island of Bulama, near the mouth of the Rio Grande, on the coast of Africa. During the voyage out, the crews and settlers of both ships were healthy, and Dr. Chisholm argues that there could be no reasonable ground for supposing that the malignant fever which after a time broke out amongst them was caused by marsh effluvia, because Bulama is surrounded by the sea, is remarkably dry, is without any marshy tracts, and in point of fact is the healthiest spot on the windward coast. But the negroes, he says, in this part of Africa being ferocious in an extraordinary degree, and said indeed to be cannibals, the settlers were prevented from erecting any sort of accommodation on shore, and were therefore obliged to live on board; and the rainy season coming on almost immediately after their arrival, and the heat being at the same time excessively great, they endeavoured to shelter themselves from both by raising the sides of the ship several feet, and covering her with a wooden roof.

According to the estimate of Dr. Chisholm, the settlers and crew, numbered in all upwards of 200 people, including women and children. These persons, he says, confined in a sultry, moist atmosphere, inattentive to cleanliness, and neglecting to sweeten the ship and to destroy the clothes, bedding, &c., of those that died, were seized with a malignant fever which produced such havoc among them that when the time for which the "*Hankey*" was chartered had expired, there were no seamen left to navigate the ship; and consequently they were obliged to proceed to sea, having on board only the captain, who was sick, the mate, one of the settlers, (Mr. Paiba,) and two

seamen. It is stated that with much difficulty they arrived at St. Jago, where they found the "Charon" and "Scorpion," ships of war, from each of which they received two men to assist them in navigating their vessel; that with this aid they proceeded to the West Indies—a voyage to England being impracticable in their state; and that on the third day after leaving St. Jago, the four men transferred to them from the ships of war, were themselves seized with the fever, two of whom died, and the remaining two were put on shore at Grenada, "in the most wretched state possible."

Dr. Chisholm further states that Captain Dodd of the "Charon" having occasion about that time to come to Grenada, and hearing of the mischief of which the "Hankey" had been the cause, mentioned that several of the "Charon's" and "Scorpion's" people were sent on board the "Hankey" at St. Jago to repair her rigging, &c.; that from this circumstance and the communication which his barge's crew had with the ship, the pestilence was brought on board both ships; and that of the "Charon's" crew thirty died; and of the "Scorpion's" about fifteen.

Dr. Chisholm thus continues his narrative:—

"The 'Hankey' arrived at the port of St. George on the 19th of February. The first person who visited the ship after her arrival in St. George's Bay was Captain Remington an intimate acquaintance of Captain Coxe, the captain of the 'Hankey.' This person went on board of her in the evening after she anchored, and remained three days, at the end of which time he left St. George's and proceeded in a drogher (a coasting vessel) to Grenville Bay, where his ship, the 'Adventure' lay. He was seized with the pestilential fever on the passage, and the violence of the symptoms increased so rapidly as on the third day to put an end to his existence. The crew of the 'Defiance,' in Blythe Port near Newcastle, were the next who suffered by visiting this ship; the mate, boatswain, and four sailors went on board the day after her arrival; the mate remained either on deck or in the cabin, but the rest went below and stayed all night there. All of them were immediately seized with the fever, and died in three days. The mate was also taken ill but recovered. The crew of the ship 'Baillies' were the next who suffered; these communicated the infection to the ships nearest them, and so it gradually spread from those nearest the mouth of the carenage, where the 'Hankey' for some time lay, to

those at the bottom of it, not one escaping in succession whatever means the captains took to prevent it.

About the middle of April the disease began to appear on shore. The first house it shewed itself in was that of Messrs. Stowewood and Co., situated close to the wharf; and the infection was evidently introduced by a negro wench who took in sailor's clothes to wash. The whole of the family were successively afflicted with it; and by them communicated to all those with whom they had any intercourse. All who from friendship, business, or duty, communicated with the diseased were themselves infected, and no instance occurred wherein the contagion could not be traced to its particular source. A few who sedulously avoided the houses where the infected actually were, escaped.

"That part of the garrison quartered nearest to where the 'Hankey' lay, were the first of this class of men who received the infection. A barrack containing nearly one-half of the 45th regiment, was situated exactly to leeward of the 'Hankey' and distant from her about 200 yards. One of the officers visited the 'Hankey,' and with two or three soldiers who rowed his boat, remained on board some time. The consequence of this imprudence was fatal to himself almost immediately after, and in a little time, to many of the men. All the officers and men were successively seized with the disease; but it proved fatal only to recruits who had lately joined.

"About the beginning of May the disease made its appearance in the detachment of Royal Artillery; a circumstance rather extraordinary, as that corps were quartered in a situation far removed from the focus of infection. It was evidently produced, however by the communication which the gunners, doing duty in Fort George, had with the 45th regiment. Of 84 people belonging to the ordnance department at that time, about 56 were seized with the disease before the 1st of July, and of these 5 died; a trifling mortality, considering the nature of the complaint. All these men, however, had been about three years in the country, and consequently suffered less from the disease, than about 27 recruits who joined the artillery in July. Of 26 of these unfortunate men who were infected, 21 died before the middle of August.

"About the 1st of June, the disease began to appear among the negroes of the estates in the neighbourhood of the town, but it did not spread much among them, nor was it marked with the fatality which attended it, when it appeared among the whites.

"About the middle of June, the disease broke out in the 67th regiment, and among the artificers and labourers on Richmond Hill. The infection was communicated by some of the latter, who had visited their friends in town labouring under

it. All were successively seized with it; but it fell heavier on the officers than the men, several of the former being young men lately arrived from Europe.

"The disease in the course of the months of May, June, and July, appeared in several distinct and distant parts of the country, where the infection was carried by persons who had imprudently visited infected houses in town.

"But the infection was not confined to Grenada alone; from this, as a focus, it spread to the other islands; to Jamaica, St. Domingo, and Philadelphia, by means of vessels on board of which the infection was retained by the clothes, more especially the woollen jackets of the deceased sailors."

Such are the alleged facts of the case as stated by its original promulgator. But the sudden creation of a *nova pestis*, wholly unlike the ordinary forms of disease prevalent in that climate, appeared to those who were acquainted with African and West Indian Fevers, so improbable that they were induced to institute an inquiry into the authenticity and truth of the facts thus set forth. In this investigation Dr. Bancroft took the lead, who has given in great detail the results of the inquiry, of which the following is a brief summary.

First, it appeared from official returns, that the island of Bulama, instead of not presenting the ordinary conditions for the generation of marsh effluvia, abounds with extensive savannahs; the north end of the island being one continued savannah, covered with long grass, and the soil being deep and rich.

"Now," says Dr. Bancroft, "it is utterly impossible that such a soil, in such circumstances should not be greatly productive of morbid exhalations, in that climate, whenever by showers or otherwise, there is sufficient moisture for their extrication, and besides these savannahs, it is incredible that the shores of such an island, and especially its harbours, should not in a multitude of places be fitted to produce and give out marsh effluvia copiously: there not being, as far as I can discover, one situation in that part of Africa, where a ship can anchor near the shore, and remain even for a week in the rainy season, without some of her crew being soon after attacked by marsh fever."

It was next shown, that the proceedings of the settlers were such as necessarily to have exposed them to the full force of whatever noxious exhalations might happen to be present. It appears that the "*Calypso*," which,

after sailing with her companion for a few days, had been separated from her by a storm, arrived at her destination fourteen days before the "Hankey." It is stated by Captain Beaver, one of the settlers, and afterwards the governor of the colony, that this interval was spent by the settlers chiefly on shore, where they erected huts and tents, wandered about where they pleased in the day, and returned or not as they thought proper in the evening; that in short nothing could be more irregular or improper than their conduct, and that in this disorderly state they were surprised and attacked by a party of Africans, and lost several of their number, and that terrified at this disaster, they re-embarked, and before the arrival of the "Hankey," had already many persons on board ill of fever.

"Those who have lived between the tropics," says Dr. Bancroft, "or who have read of the numerous instances of mortal fever produced by sleeping a single night on shore, will not be surprised that, after such conduct, it should have been found, as is mentioned by Captain Beaver, that the 'Calypso' had many persons ill of fever."

The two ships having rejoined, they anchored together in the Bissao road, a place, says Dr. Bancroft, where the coast fever is known to prevail in the rainy season, which had then recently begun. They remained here fourteen days, and during this period fever broke out in the "Hankey" also. Instead of referring the disease of the "Hankey" to the same cause as that which had produced it in the "Calypso," it was conceived according to the received doctrine of that day, that the "Calypso" had infected the "Hankey;" and Captain Beaver himself appears at first to have been of that opinion; "but there is good reason," says Dr. Bancroft, "to believe that he did not long retain this notion; but was soon convinced from personal observation that contagion had no influence in spreading the fever, or occasioning the mortality which resulted from it." A brief history of the two ships subsequent to this period will suffice to show the credibility of the evidence as to importation.

Captain Beaver gives the following account of the condition in which he found the "Calypso," on the arrival of the "Hankey."

"Nothing," he says, "was heard but mutual reproaches from the people of the 'Calypso.' The colonists accused the members of the council in the ship, of a want of attention to their comfort and accommodation. They were tired out with the length of their voyage; irritated by the loss of their friends by the recent attack of the natives, and the disappointment of their hopes; the major part of them had become drunken, lazy, dishonest, and impatient, and the setting in of the rainy season, with the prospect of great fatigues and privations to be undergone during that season, had probably contributed not a little thereto. In this state of mind a very considerable number of them resolved to abandon the colony; and accordingly sixteen of them separated almost immediately; and others, to the number of 147 (among whom were the Governor, Mr. Dalrymple, and the Lieutenant-Governor, Mr. Young), sailed from Bulama in the 'Calypso' on the 19th of July, for Sierra Leone, and thence to London; where they arrived in the middle of November, excepting about 40 who died from sickness."

On the departure of the "Calypso," the remaining colonists elected Captain Beaver to be Governor, and this gentleman gives the following narrative of the subsequent course of events.

"The first and principal undertaking of the new colonists," he says, "was that of building a block house for their protection and accommodation upon the summit of a hill near the harbour, which was then covered with a thick forest. But having no sort of accommodation on shore, they were constrained to live on board the 'Hankey'; and the better to defend themselves from the rains, they erected a covering on the ship's deck. Their only shelter on the island was a little tool-house, and as the heavy rains made it impossible to dress their victuals on shore, the working people were landed on the island at daylight every morning, and brought back on board to breakfast at 8 o'clock, carried back to their work at 9, brought on board to dine at noon; again relanded at 2 p.m. to resume their labours, and finally brought on board to sleep at sunset. In this manner they continued to work through the rainy season, (which ended about the 15th of October), and afterwards until the beginning of November, when the time being expired for which the 'Hankey' had been chartered, it became necessary to land the colonists with all their stores, to enable the ship to prepare for her departure, although the block-house could not be finished till the month of February.

"On the 23rd of November the 'Hankey' sailed from Bulama. On the 26th she anchored at Bissao, and sailed thence for England on the 3rd of December; but in the night of the 4th she grounded on a sand-bank, near the Island of Warang,

which made it necessary to send a part of her crew in the pinnace (an open boat) to Bissao for assistance, and these men after hard rowing for two days and nights, returned with a schooner and a long boat, and with their help, the ship was again made to float; but on this day all the people who came from Bissao in the pinnace were taken ill. On the 13th the 'Hankey' was brought back to Bissao to refit; but finally sailed thence on the 21st of December, and anchored in St. Francis Bay, at St. Jago, on the 26th of that month."

Such appears to be a faithful narrative of the events that befel both ships, in which it seems difficult to discover anything extraordinary; much less anything justifying the notion of the generation of a new and peculiar plague; for even the excessive mortality from which the settlers suffered, experience proves to be but too common among the first adventurers to a new colony.

"Those," says Dr. Bancroft, "who were eye-witnesses of the disease which proved thus fatal, recognised in it nothing of Dr. Chisholm's *nova pestis*, and saw no indication of a peculiar, original, foreign plague, utterly unknown before! A fever of infection heightened to pestilential violence! A jail fever between the tropics!"

On the contrary, instead of recognising anything peculiar and extraordinary in the disastrous fate of so many of his companions, Lieutenant-Governor Young, who was with the colonists in the "Calypso" when the fever first broke out at Bulama, accompanied the survivors on their return to England, and witnessed the deaths that took place during the home voyage, declares in so many words "that this fatal fever was the coast fever;" that "it was intermittent," and that "it was not infectious."

In like manner Mr. Paiba, another of the colonists, also an eye-witness of the fever at its first outbreak, and during its whole progress, a witness to whom Dr. Chisholm appeals as his authority, but who positively contradicts some of his material statements, bears testimony to the same effect as Lieutenant-Governor Young, in the following words:—

"Concerning the sickness which carried off the colonists, both at Bulama and Sierra Leone, and in the home passage of the 'Calypso,' it may be remarked once for all, that it was by no means of one kind, as the readers of Dr. Chisholm would be

led to suppose. Few, if any, escaped altogether; some had regular intermittent fever which is the fever of the coast; others had a violent fever (probably says Dr. Bancroft, marsh fever aggravated by accessory causes, such as being caught by heavy rain, drunkenness, fatiguing exercise, or labour in the sun, &c.) which terminated favourably or fatally in one, two, three, four, five or six days, or which lingered out after its first violence as many weeks; some had diarrhœa and dysentery; and others fell martyrs to the indiscreet use of opium and spirits as preservatives."

These statements as to the true nature of this fatal fever, were confirmed in every particular by the independent evidence of Dr. Winterbottom, physician to the Sierra Leone Company, who was on the spot when the "Calypso" arrived, and appears to have attended the sick, and who gives the following decisive testimony:—

"The fever," he says, "which carried off so many of the settlers at Bulama precisely resembled the endemial remittent fever of Sierra Leone. The 'Calypso' after leaving Bulama, called for refreshment at Sierra Leone, where she remained about six weeks, during which time upwards of forty of the crew and passengers died of the remittent fever, though unattended with any appearance of peculiar malignity."

Dr. Bancroft observes on this clear and authoritative evidence:—

"Against such testimony, the unsupported assertions and suppositions of Dr. Chisholm, who never was on the coast of Africa, nor personally acquainted with any of the facts in question, must be of no value."

But if the assertions of Dr. Chisholm as to the peculiar and extraordinary character of the fever in question were without any real foundation, still worse than valueless appears to have been the evidence he adduces with a view to prove the communication of this new pestilence by direct contact with uninfected ships and persons.

In the narrative already quoted it will have been seen that Dr. Chisholm asserts in the most positive manner that a mortal fever was communicated by the "Hankey" to the "Charon" and "Scorpion" ships of war, and that the "Charon" lost by this pestilence thirty of her crew, and the "Scorpion" fifteen.

"By the publication," says Dr. Bancroft, "of such apparently decisive, though fictitious, instances and proofs of a most power-

ful and destructive contagion, on board the 'Hankey,' joined to others of equal value, which were stated by Dr. Chisholm to have occurred after her arrival at Grenada, we cannot wonder that many persons were so far misled as to believe in the generation and importation of a new and 'direfully' contagious fever by that ship; for at first the 'Hankey,' and not the 'Calypso,' was represented as the parent of this monstrous and dreadful production. Fortunately for the cause of truth, the falsehood regarding the 'Charon' and 'Scorpion' was detected, and laudably exposed by Dr. Trotter, who happened to be then surgeon to the 'Vengeance,' ship of the line, one of Admiral Gardner's squadron, under whose protection the homeward bound West India summer fleet (of 1793) was then returning to England. He informs us at page 327 of the 1st vol. of his *Medicina Nautica*, that on the 22nd of August, a ship, one of the fleet, 'lost her foremast in a squall of wind, and received other damage, when the admiral made the signal for the 'Vengeance' to take her in tow. The ship proved to be the 'Hankey' from Grenada and Bulama. Captain Thompson sent carpenters on board with the necessary stores to assist in repairing her losses: they remained for three or four days, *but no sickness followed,*' &c. Dr. Trotter afterwards mentions, how from this and other circumstances, he was induced to make inquiries concerning Dr. Chisholm's account of the consequences of the 'Hankey's' intercourse with the 'Charon' and 'Scorpion,' particularly from Captain Dodd, who 'had his broad pendant' in the former ship, and of Mr. Smithers who was her surgeon: from them, says Dr. Trotter, I have copied the following narrative of their transactions with the 'Hankey,' viz., 'When the squadron under Commodore Dodd, came to St. Jago in 1793, the 'Hankey' lay there in great distress for want of hands. The fever was now overcome; Mr. Smithers saw two men who had lately recovered. \* \* \* The 'Charon' and 'Scorpion' sent two men each to assist in navigating her to the West Indies. The 'Hankey' at this port was cleansed, washed with vinegar, and fumigated. No fever appeared in either of the men of war in consequence of this communication. They arrived at Grenada in perfect health,' &c. Dr. Trotter adds, 'It is probable from these facts, that the 'Hankey' did not import the infection that produced the Grenada fever. It is also doubtful how the effects left in the 'Hankey' could produce the fever, for the bedding was thrown away, and what clothing remained had been aired, and probably had scarcely been in contact with the body after being sick. Mr. Smithers was examined before the (lieutenant) governor of Grenada on the subject, and gave his opinion decidedly, that the 'Hankey' did not communicate this fever to the colonists.'

"From this statement," continues Dr. Bancroft, "and other

proofs, it has been unquestionably ascertained, that every part of Dr. Chisholm's account, which asserts the communication of any disease from the 'Hankey' to the 'Charon' and 'Scorpion,' was a mischievous falsehood, fabricated without the smallest foundation or particle of truth, since the latter ship did not lose a single man during her whole voyage, and the 'Charon' lost only four from causes described by Dr. Trotter, and wholly foreign to the 'Hankey.' "

With a view to sustain the allegations of Dr. Chisholm respecting this alleged case of importation, Sir William Pym states that the health of Jamaica previously to the arrival of the "Hankey" was good, and in support of this statement quotes the authority of Dr. Hunter as to the salubrity of Fort Augusta and Stoney Hill.

"On consulting Dr. Hunter's observations on the diseases of the army in Jamaica," says Dr. Gillkrest "we find it is indeed true that that writer speaks favourably of the two above-mentioned localities; but they are expressly mentioned among exceptions, and by way of contrast with the sickly, nay deadly state, of other parts of the island during the period referred to.

"Thus at p. 11, Dr. Hunter says, 'Four regiments were sent from England in 1780 to Jamaica; they arrived there the 1st of August, and before the end of January ensuing, not quite six months, one-half of them nearly were dead, and a considerable part of the remainder were unfit for service.'

"At p. 57, he says, 'An average of the number of sick during three years and a half, in which are included the convalescents, gives one-third of the army unfit for service at the time of the greatest sickness, and one-eighth at the time of the least sickness. The average of deaths annually upon the whole is nearly one in four, and of discharged men about one in eight, which, together, makes the loss three-eighths of the whole. In less than four years there died in the island of Jamaica 3,500 men; those that were discharged amounted to one-half of that number, which make in all 5,250 men lost to the service in that short period of time from the climate and other causes of mortality, without a man dying by the hands of the enemy.'

"Speaking of the symptoms of the disease, p. 64, he says, 'The vomiting is sometimes constant and violent, especially in the worst kind of the disease, and the blood being frequently in a dissolved state is forced into the stomach and thrown up, forming, what has been called by the Spaniards the black vomit.'

"Is it then credible," asks Dr. Gillkrest, "that with this book before him the British Superintendent of Quarantine should write the following, at p. 59 of his second edition?

" 'These quotations, from the highest authority, prove that

for several years before 1793 there was no instance of that species of disease prevailing epidemically in the West Indies, which is characterised by black vomiting, &c.’”

As a contrast to Sir William Pym’s account of the general salubrity of the West Indies, we may cite the description given by Dr. Fergusson on personal examination of the island of Grenada in particular, into which the “Hankey” is asserted to have imported the epidemic in question. According to Dr. Fergusson there is no necessity to have recourse to the supposition of importation, to account for the existence of Yellow Fever in this island, there being few places in which the endemial causes of this disease are more abundant. After describing the ridges of Richmond Hill, Cardigan Heights, and Hospital Hill, “the base of which “touches upon the carenage, and is foul, swampy, and “most unwholesome,” Dr. Fergusson says :—

“The carenage itself differs little from the pestiferous creeks which bear that name in the West Indies. Its channel winds for about nine hundred yards round the bluff of Fort George so as to be invisible from the sea till you are actually in it, when it presents an oblong cul-de-sac, enclosed on every side by the high grounds of the Hospital and the Richmond Ridge, hanging directly over it. The marsh below Hospital Hill is within less than half a mile on the north west: the swampy margin of the lagoon, as it is called, (a conglobate appendage of the carenage, but separated from it by a reef under water,) is within a short distance of the south-east; and the bowsprits of the shipping, even where they lie at anchor, overhang in many places the muddy uncleared banks of the carenage.

“No winds but such as come in eddying gusts through the chasms of the hills can ever fairly sweep the surface of the waters at the anchorage, which from the shape of the harbour, derive scarcely any movement from without; and when the port is crowded with vessels, obstructing all ventilation by lying after the manner of a carenage, as close together as they could be supposed to do in a dock, it is difficult to conceive, amidst the stagnation of the elements and combination of pestiferous sources, a spot more likely to generate, under a tropical sun, the most aggravated species of Yellow Fever.”

In further confutation of Dr. Chisholm, Dr. Stuart, who attended Captain Remington during his last illness, directly contradicts the account given of the extraordinary violence of the disease under which his patient

laboured; attributes his disease not to a mortal contagion, but to a cold caught on his passage; and states that the symptoms, instead of justifying Dr. Chisholm's description that they "increased so rapidly, as on the third day "to put an end to his existence," were all so very moderate, that when he left his patient at the end of three days, he did not even suspect danger.

With respect to Dr. Chisholm's account of the seizure of the crew of the "Defiance" in Blythe Port, and of the crew of the ship "Baillies," Dr. Bancroft shows that these statements are made without the slightest authority, and that they are in the highest degree improbable.

"We have seen," he says, "that the 'Calypso,' which is supposed by Dr. Chisholm to have infected the 'Hankey,' and whose fever consequently must have been the same, was not suspected at Sierra Leone to have communicated any disease there, nor to have had any other than a marsh fever on board. And we have seen that she returned thence to England, after suffering a mortality greater in proportion to her stay in Africa than that of the 'Hankey;' and it never was even suspected that either her crew or her surviving passengers, with all their bedding and effects, and those of the dead also (though landed and dispersed in Great Britain,) had introduced any sort of contagion, there being fortunately no person here to excite groundless alarms, by inventing and propagating falsehoods as in Grenada. In regard to the 'Hankey' we have every reason to believe that no other than marsh fevers or fevers from cold, fatigue, intemperance, and other similar causes, had occurred on board of her during the whole voyage. It appears, moreover, that she was three times cleansed and fumigated, viz., at Bulama, Bissao, and St. Jago; and at the latter of these places (at least) she must have been free from any contagion; for otherwise, her unrestrained intercourse with the inhabitants would have excited disease in some of them; which it did not, nor in any person on board the 'Charon' or 'Scorpion,' nor in the four sailors obtained from those ships by Captain Coxe; who, during the subsequent part of the voyage, could not have escaped it, if any had existed capable of producing such wonderful effects as Dr. Chisholm has supposed in Grenada. It appears certain that everybody on board the 'Hankey' was well when she arrived at Grenada, and continued so during her stay there and also during the whole of her voyage to England, which is probably more than could be said of any other ship then at Grenada; so that even if it were ascertained that an importation of febrile infection had then taken place, every other ship ought to have been sus-

pected of it before the 'Hankey,' coming, as she did, from a place where there is good reason to believe no contagious fever had ever existed."

It is unnecessary to follow this strange narrative farther. All the other statements made by Dr. Chisholm appear to be equally without any real foundation in fact, and the following official declaration of the governor of the colony and master of the ship is decisive of the whole question:—

"The mortality," says Captain Beaver, "which took place in the island of Bulama, and on board the 'Hankey' after her departure from it, was in this country, (England,) called the Plague or Bulama Fever, by those who were inimical to the success of our enterprise; and such serious representations were made on the subject, as produced an order from the privy council to sink that ship, though on further inquiry it was not carried into effect; and the ship was restored to the owners, after having sustained very considerable loss, by the industry with which certain interested people kept up the report of the malignity of the distemper, which it was said that ship brought home, and for which there was not the shadow of a foundation."

\* \* \* This report (of a pestilential fever supposed to have been introduced by the 'Hankey,' from Bulama to Grenada,) was for a considerable time believed; the 'Hankey' was sent to Standgate Creek to perform quarantine, and orders were given for sinking the ship and cargo; however, on examination, the falsehood and malignity of this report being proved, this order was confined to the Bulama baggage only."

We have dwelt the longer on this alleged case of importation, because it is the first so boldly and circumstantially affirmed in modern times, because it was for a time extensively believed, and because it has served as a model for the kind of statements made in all the later reputed instances of a similar event.

With the same confidence, as in the case of the "Hankey," Dr. Chisholm asserts that the "General Elliott," East Indiaman, imported the contagion of Yellow Fever into Fort Royal, Martinique, in June 1796. But in this instance, Dr. Chisholm was equally unfortunate in the evidence which he adduces in support of the allegation, as in the case of the "Hankey." His representations and statements have received direct contradiction and refuta-

tion by Dr. Fergusson, who happened to be the surgeon of the "General Elliott" at the time in question, and was on board of her in that capacity during her voyage, and on her arrival at Martinique.

"This" (the General Elliot) "was one of the ships appointed in the spring of 1796," says Dr. Fergusson, "to take out the scattered remains and detachments of Admiral Christian's armament, that had been so severely handled by the elements the preceding winter. It so happened that I being then on my way to join the 67th regiment in Saint Domingo, was ordered on board of her to take charge of the detachments of the Buffs, 38th, and 60th, she was carrying out. I also acted as surgeon to the ship's crew, and can declare that when we landed at Martinique, there was not a single sick human being, except the ship's carpenter, who was far gone in consumption; nor had there been the smallest illness amongst us from the day of our sailing at Portsmouth, except a very few of the slightest calentures (I had one) when we first entered the tropics, none of which endured more than twelve hours."

The outbreak of Yellow Fever at Cadiz, in 1800, after the arrival of the ship "Dolphin," has been referred to as an instance of the importation of the disease. Of this case Dr. Bancroft furnishes the following account:—

"In the summer of 1800, the Government, as well as the inhabitants of Cadiz, appear to have adopted the belief of an importation of the supposed contagion from America; and a ship called the 'Dolphin,' belonging to Baltimore, was generally and decidedly selected and accused as having been the vehicle of this mischief: and reports were fabricated, by which three persons were stated to have died of Yellow Fever on board the 'Dolphin' during her passage, and what had been supposed to be the first cases of the fever at Cadiz, were declared to have occurred in different individuals, who had all directly communicated with the 'Dolphin,' or some of her crew: and other sailors belonging to the same ship were said to have found their way up the Guadalquiver, through St. Lucar (in which town, however, the disease did not appear until the middle of September), and, by lodging in the suburb of Triana, at Seville, to have produced the Yellow Fever there some days before its appearance at Cadiz. These stories, in point of detail and seeming accuracy, were such as Dr. Haygarth, by his letter of the 23rd of May, 1799, solicited Professor Waterhouse to procure for him respecting the importation of Yellow Fever into Philadelphia, &c., and they were circulated generally and with great confidence, so that Don Pablo Valiente, Intendant of Cuba, who had chartered the 'Dolphin' to bring

himself and his family to Spain, was, notwithstanding his rank and connexions, arrested upon a criminal charge, tried before the Royal Audiencia at Seville, and after a full investigation, and eleven months' imprisonment, fully and honourably acquitted of having introduced the Yellow Fever at Cadiz; and he was, probably as a compensation for the injustice he had suffered, afterwards promoted by the Government. In the course of this prosecution, it was juridically proved that the Yellow Fever had not appeared at the Havannah, whence the 'Dolphin' sailed in May, 1800, until sometime after her departure; and though she touched at Charleston on the 2nd of June, and sailed thence on the 10th of that month, it was (in consequence of an application from the Spanish Government) certified unanimously, at an extraordinary meeting of the Medical Society of South Carolina, on the 25th of April, 1801 (22 respectable physicians being present), that, 'to the best of their knowledge, no Yellow Fever had existed in that town or in the port of Charleston prior to the 20th of June in the year 1800.' They also declared, on the ground of specified facts, their conviction that the disease in question had never been propagated by contagion. It was also proved, and particularly by the testimony of Don José Caro, a Spanish physician, who had returned as a passenger on board the 'Dolphin,' and was examined on oath by the judges at Seville, that the diseases of which the three sailors had died on board of that ship were not of the nature of Yellow Fever, but different diseases, of which an account was given. It was, moreover, proved that no symptoms of the Yellow Fever had appeared in any person on board the 'Dolphin,' and consequently that the disease could not have been introduced by that ship; Dr. Arejula has therefore deemed it proper to reject the stories concerning the 'Dolphin,' and to confess that it was impossible to ascertain whence the epidemic was derived."

On no better evidence it was asserted in the like confident manner, that Yellow Fever was imported into Gibraltar in the year 1810, by certain vessels arriving in Gibraltar bay from Carthage.

"During the prevalence of contagious fever at Carthage in 1810," says Dr. (Sir William) Pym, "four transports from that port anchored in the bay of Gibraltar on the 10th of September; they were immediately put under the restraint of quarantine. On going alongside I was informed that one man was already dead, two were dangerously ill, and several others had been attacked with fever which showed itself in all the four transports in the course of a few days."

Though according to this statement these transport

were immediately put under the strictest quarantine, and continued under it during the whole time of their stay at Gibraltar, and though "they were anchored," says Dr. (Sir William) Burnett, "at the distance of at least half a mile from the garrison, and probably more," nevertheless, Yellow Fever broke out in the garrison.

"On the 20th of October," says Dr. Pym, "forty days after the arrival of the transports, in consequence, as I must suppose, of a breach of quarantine regulations, which, however, could not be detected, a Minorcan family in the south district belonging to the dockyard, was attacked with the disease."

In commenting on "this gratuitous supposition" of a breach of quarantine, Dr. Bancroft observes, "If quarantine regulations in such a case could be frustrated even when enforced by Dr. Pym as their superintendent, and when he was fully convinced of the presence of a dangerous contagion, it certainly must be useless to employ, and highly dangerous to rely upon them at any time."

"On visiting this Minorcan family," continues Dr. Pym, "I found that the disease which we so much dreaded, actually existed; six out of seven persons had been attacked in one house, and three other families, neighbours, who had visited them, as also a Spanish priest," &c.

On inquiring into the correctness of this statement, Dr. (Sir William) Burnett adduces conclusive evidence to show that the Minorcan (whose name is Jacinto Ray) was not the first person attacked, and that he had no communication with the transports from Carthagea.

"I have proved," says Dr. Burnett, "by the solemn declarations of Jacinto Ray upon oath, and also by the concurrent testimony of Messrs. Donnet and Amiel, that this person had no communication with the transports from Carthagea, and also that he was not the first person attacked; that no Spanish priest had attended Jacinto Ray during his illness, and consequently none could have received infection from this source; that all the men taken ill in the barrack room of the 7th Veterans, were within the walls of the town in a very populous part, and that the fever existed in the hospital of the 7th Veterans, from the 11th of October—that is at least nine or ten days before the occurrence of Dr. Pym's first case.

Dr. Bancroft sums up his examination of the trust-

worthiness of the statements and evidence adduced by Sir William Pym in reference to this alleged case of the importation of Yellow Fever from Carthagera into Gibraltar in the following words:—

"The subsequent part of Dr. Pym's account of the progress of the disease has been minutely noticed and abundantly refuted by Dr. Burnett, between pages 442 and 450 of his work; and to them I beg to refer, because as he has anticipated every thing which I could offer on the subject and with some facts of which I had no knowledge, it becomes me to allow him the credit which is his due, for the ability with which he has supported the cause of truth. \* \* \* \* \* Whoever will attentively peruse that part of Dr. Burnett's work, must I think be fully convinced that Dr. Pym has entirely failed in his attempt to make it probable, or even possible, that any imported contagion, or any other than local or atmospherical causes, should have produced any of the cases of fever at Gibraltar in 1810, or that, when produced, they manifested any contagious property in a single instance."

Gibraltar has been the frequent seat of Yellow Fever epidemics, and on every occasion the local authorities have endeavoured to prove that the disease was not endemic but imported. With reference to the epidemic of 1813, for example, though the Gibraltar Board of Health attributed the sickness to local causes, the Lieut.-Governor published a statement that the contagion was received from Cadiz. The authority on which this story rests is chiefly that of a Spanish practitioner (Cortes), the leading witness (see Appendix II., p. 246) in the attempt to prove the importation of the subsequent epidemic of 1828 by the ship "Dydden," hereafter to be noticed.

The statement is, that a vessel called the "Fortune" arrived in Gibraltar from Cadiz on the 11th of August; that one of the crew who was ill was sent to the Catholic Hospital, and died on the 19th of the same month; that a native of Paris, who had come to Gibraltar in the same vessel, died on the 3rd of September; and that from the 3rd to the 11th of September nine persons died in the neighbourhood of the hospital.

On the other hand, it is affirmed by Sir James Fallowes, the principal medical officer of the British army at Cadiz, that on strict inquiry he found that nothing un-

usual had taken place respecting fever in Cadiz before the 11th or 12th of September; that until the end of August the health of that city was excellent; and that it was impossible, therefore, that the vessel in question could on the 11th of August have imported the disease from Cadiz to Gibraltar, when it did not exist in Cadiz at the time she sailed, nor, according to the medical statements, till more than a month afterwards.

It was in like manner affirmed that the Yellow Fever epidemic at Barcelona in 1821 was imported from the Havannah by the "Grand Turk," and a great amount of misrepresentation as to the real facts was promulgated at the time. On an examination of the circumstances of this case, it appears that when the "Grand Turk" left Havannah no Yellow Fever was prevailing there, all the vessels having left with clean bills of health; that no case of the disease occurred on board during her passage; that no cases appeared at Barcelona until 33 days after her arrival; and that the disease first broke out on the 1st of August on board a Neapolitan polacca, which had been lying in the foul and pestiferous port of Barcelona during the previous four months. The Committee of Physicians further state, that during the months of February, March, April, May, and June preceding the arrival of the "Grand Turk," "there were prevalent, both in Barcelona and Barcelonetta, fevers with black vomit, yellowness, and other alarming symptoms, such as happen sporadically more or less every year."

"We cannot," they say, "adopt the idea of the importation of the fever from the Havannah to this port, because it does not rest on any certain fact, nor any satisfactory reason, and because we have before our eyes palpable local causes which undermined the health of the city, and in concert with the season and certain meteorological conditions, occasioned the epidemic."

This view is confirmed by a proclamation of the municipal authorities of Barcelona, dated the 18th January, 1822, who state—

"From all the writings and medical discussions which we have seen up to this day, it results that the putrefaction of the waters of the port has either been the origin or has contributed notably to the development of a disease of painful memory to our country."

In the year 1823 an opinion was advanced by Sir Gilbert Blane whose eminence and official position gave to any statement made by him the weight of authority, that the "Bann" sloop of war, sailing from Sierra Leone, introduced Yellow Fever into the island of Ascension. This case was examined by Sir William Burnett, who published an official report upon it, from which it appears that Sir Gilbert Blane was in error as to the state of the health of the crew of the "Bann," when she left Sierra Leone; that disease of precisely the same character as that alleged to have been imported by the "Bann" had existed and been prevalent in the island at former periods; that after the most careful inquiry it was found impossible to trace the fever in question directly from the "Bann," to any individual of the garrison of Ascension; and that the first person attacked was certainly not known to have been in the ship, or in contact with the sick.

The case of the "Eclair," and the history of the Epidemic Fever which occurred at Boa Vista in 1845, have been declared by high medical authority to afford "conclusive evidence that Yellow Fever is sometimes imported." It will therefore be necessary to make a careful examination of the circumstances relative to this Epidemic.

It has been affirmed, and generally credited, that unusual effort has been made to ascertain the facts of this case under circumstances more than commonly favourable to the discovery of the truth. Two official Reports respecting it, drawn up after personal inspection on the spot, have indeed been presented to Parliament—one by Dr. McWilliam, and the other by Dr. King; and several official notices of these Reports have been published; but the evidence on which these two Reports were founded was not collected until some time after the cessation of the epidemic. The statements of witnesses, for the most part poor and ignorant, many of whom had a direct interest in establishing the importation of the disease by a British ship, have been admitted implicitly, even with respect to dates and circumstances not of recent occurrence, and without due examination of the credibi-

lity of their testimony ; and on all material points the reporters have arrived at directly opposite conclusions.

On a review and comparison of the whole of the statements which have been made with respect to this case, it appears that the steam-ship "Eclair," with a crew of 140 officers and men, proceeded in 1844 to the coast of Africa, and was stationed for upwards of four months (130 days) at the island of Sherboro, with a view to blockade the eastern outlet of the passage at Shebar. This place is considered one of the most unhealthy on the African coast ; vessels remaining near the island very rarely escaping an outbreak of Yellow Fever on board. The land is represented as low lying, some parts being marshy, and the rest thickly wooded, and abounding in rank vegetation.

According to the account of the surgeon of the "Eclair," Mr. Maconchy, the ship on this occasion was anchored at the mouth of the river, in a position where she "was surrounded with filthy-looking river-water, urged backwards and forwards by the tides through extensive tracts of mangrove bushes." The fresh water used on board was also bad, holding in solution a quantity of offensive vegetable matter, which produced in some of the crew attacks like mild cholera. The men, in parties of from 30 to 40, were often sent up the river on boating expeditions, where they remained for seven or eight days at a time exposed, "whether they slept on board or ashore, perhaps after a hard day's labour, to all the exciting causes of fever, and a tainted nocturnal atmosphere, in the rainy season, heavy weather having set in, and the men constantly getting as wet as possible."

The danger of this boating service is thus stated by Dr. King:—

"The duty in boats up African rivers involves considerable risk at any time of the year, but it can never be practised in the rainy season without endangering the health and lives of all who are employed, and such were evidently the sad consequences of the boat expeditions of the 'Eclair.'"

The crew, according to Mr. Maconchy, in addition to this dangerous service, and the dreariness and monotony of the situation, were exposed to another depressing agency, "from seeing the prizes of other ships passing frequently

to Sierra Leone, whilst they considered themselves out of the reach of such good fortune."

Another cause was probably in operation even at this time, namely, the foul condition of the ship, as will hereafter appear.

Under these circumstances, fever broke out on board the ship, and proved fatal to ten of the crew; eight of the ten deaths being considered by the medical officers as directly consequent on the boating expeditions. Though there were other and severe cases of sickness on board, these deaths appear to include the whole of the ship's mortality during her stay at Sherboro, a period, as has been stated, of above four months.

In the month of July the "Eclair" left this station, returned to Sierra Leone, and anchored in the harbour, where she appears to have remained 13 days. This happened to be the rainy season. The crew went on shore, where several of them remained at night unable to reach the ship from being in a state of helpless intoxication.

The consequences were soon apparent. While the ship remained in the harbour, fever again broke out on board with great violence, and continued without intermission during this and the following month. In this sickly state she again left Sierra Leone, proceeded northward in company with another ship, the "Albert," and anchored in the Gambia on the 10th of August " (one of the most unhealthy months at that place)," where she remained until the 15th. All this time, the fever steadily increasing, she arrived on the 21st of August at Boa Vista. She had now lost, since leaving Sherboro, 13 more of her crew, making in all, from the first outbreak of the disease at Sherboro, 37 attacks and 23 deaths; that is, 1 in 6 of the crew had died.

On anchoring in the harbour of Boa Vista, pratique was at once offered to her commander, Captain Estcourt, but he replied that he could not think of accepting it until he had communicated the state of his vessel to the authorities on shore. After some deliberation the Governor-General consented to the landing of the ship's company, in the hope that the formidable disease, by which so many had already perished, and so many others were still placed

in imminent danger, might be checked. Accordingly the crew, both the healthy and the sick, were sent to a Fort on an islet a mile distant from the town (Porto Sal Rey), and the officers were lodged in the town itself. This took place on the 31st of August.

The hope of benefiting the crew by the change of their quarters from the ship to the land was not realized. On the contrary, the sickness continued to increase with so much virulence that, at the end of the third week after the arrival of the ship at Boa Vista, no fewer than 60 fresh cases were added to the sick list, and some deaths took place nearly every day.

In this state of things a consultation of the medical officers was held on the condition of the crew, the result of which was a recommendation that the ship should immediately proceed to Madeira, and if the fever received no check, that she should go on to England. In conformity with this advice, the whole of the crew, the sick as well as the healthy, were forthwith re-embarked, and the ship sailed from Boa Vista on the following day, namely, the 13th of September.

The sequel to this sad narrative shows that no improvement took place during the passage of the "Eclair" to Madeira, where she was refused pratique. She therefore proceeded next day on her voyage to England, and anchored off the Isle of Wight, at the Motherbank, on the 28th of September, having lost, since sailing from Boa Vista, 12 more of her crew. Thus in the short space of 37 days, that is, from the time when she anchored at Boa Vista on the 21st of August, till her arrival at the Motherbank on the 28th of September, there occurred no less than 90 attacks and 45 deaths, including the death of her excellent and devoted captain.

On her arrival in England the ship was put in quarantine, and remained under the direction of the Privy Council until the 31st of October.

On the day following her arrival, Dr. Richardson proposed that the sick should be immediately removed to a wing of Haslar Hospital, to be appropriated exclusively for them; stating, that in his opinion, if the sick were placed in well-ventilated wards, with fresh bedding, and

the other means of cleanliness afforded by an hospital, there would be no further risk to the attendants than would occur in wards set apart for cases of typhus fever.

To this advice, Sir William Pym objected, and instead of allowing the removal of the sick, he ordered the vessel, with the whole of her crew, to proceed from the Motherbank to the Foul Bill Quarantine Station at Standgate Creek, which place she did not reach until the afternoon of the 2nd of October, that is, four days after her arrival at the Motherbank, where they remained six days more before their removal into another vessel. Thus were all on board detained close prisoners in a pestilential atmosphere on the shores of their native land; their anticipations that at length they should quit the scene of such terrible sufferings, and of so many horrors, their hopes of life and health, totally destroyed. The consequence was, that within these ten days, five more deaths took place, nor was it until the Lords of the Admiralty declared their conviction that the only means of preserving the lives of the survivors of the crew would be the entire removal of every individual from this ill-fated ship, that they were permitted to quit it. Their removal took place on the 8th of October, after which event two more deaths occurred, one of them being that of the pilot who took the vessel from the Motherbank to Standgate Creek.\*

As already stated, official inquiries were directed to be made into the causes of this extraordinary mortality, from which it appears:—

That there was nothing peculiar in the disease itself. The medical and other officers of the ship, as well as the medical and other officers at Boa Vista, that is, all competent witnesses who actually saw the disease, concur in stating that it was nothing more than an aggravated form of the common endemic fever of the African coast; a view which is decisively confirmed by the original

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\* A striking contrast to this treatment of the crew of the "Eclair" is exhibited in the case of Her Majesty's frigate, the "Arethusa," which recently (Feb. 14, 1852) arrived at Plymouth from Lisbon, having on board cases of small-pox. Instead of putting the ship in quarantine, and confining the healthy in the same poisonous atmosphere with the sick, wiser counsels on this occasion prevailed, and more humane measures were adopted. On the advice of Dr. Rae, Inspector of the Royal Naval Hospital, the sick, twelve in number, were immediately removed to that establishment, and of these, two died, without any communication of the disease.

description of the disease in the medical journal of the ship, and by post-mortem examination.

In opposition to this generally-received opinion, however, Sir William Pym promulgated a statement that, in addition to the common African fever, the celebrated *nova pestis* of Dr. Chisholm had been introduced into the vessel by a passenger taken on board at Sierra Leone; this disease being, as he represents, a fever *sui generis*, known by the name of the African, Bulam, Yellow, or Black Vomit Fever, attacking the human frame but once, and differing from the common remittent fever in being highly contagious.

That the doctrine on which Sir William Pym's assertion rests met with little countenance from medical authorities is apparent from the statement of Sir William Burnett, who says:—

"The whole of this, as regards the peculiar properties of the disease, called by Sir William Pym, Bulam, &c., is a gratuitous assumption on his part, and, in my opinion, has no foundation in fact; and in my view of this part of the subject I am supported by nineteen twentieths of the medical officers of both services, who are of opinion with myself that the more ardent form of Yellow Fever is a mere modification of the bilious remittent\* so extensively known all over the tropical regions."

He adds: "The fever which prevailed in the 'Eclair,' was unquestionably a remittent fever, originating in marsh miasmata, and the exposure of the men in boats during rainy weather."

"The true yellow fever," says Mr. Watson, of Port Royal, Jamaica, "is, in point of fact, of rare occurrence in this place, in an epidemic form. Fevers make their appearance from the deadly black-vomit pestilence, to the innocent ephemeral paroxysm, so blended and shading off from the most fatal to the least so, as to require the treatment to be equally varied. Diseases which for the first day or two have seemed trifling cases of common fever, have frequently all of a sudden assumed the most lethal aspect of yellow fever, and hurried their victims to the grave in a few hours."

Dr. King and Dr. Stewart, in official Reports upon this case, state their concurrence with Sir William Burnett. Dr. McWilliam, on the other hand, is of opinion that the disease, though primarily an endemic

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\* In confirmation of this assertion of Sir Wm. Burnett, and in opposition to the statement (*See* p. 126), that Intermittents are "unknown" in Gibraltar, we subjoin the following:—

\* RETURN of the Annual Number of Admissions into Hospital from the Ordnance Military Corps stationed at Gibraltar, for Intermittent, Remittent, and Yellow Fever respectively, with the Average Strength of the Force in each Year, from 1812 to 1851 inclusive :—

Years.	Strength of Force.	Intermittent.	Remittent.	Yellow.	Remarks.
1812	804	5	28	..	The far greater proportion of all the fevers at Gibraltar have been returned as "simple continued fevers;" but many of these approximated in 1813, 1814, 1818, 1828, and other years, to Yellow Fever.
1813	775	1	12	..	
1814	750	..	3	73	
				called epidemic fever.	
1815	569	..	2	..	
1816	610	1	3	..	
1817	..	..	1	..	
1818	470	..	..	..	
1819	453	2	..	..	
1820	457	5	..	..	
1821	451	5	..	..	
1822	428	6	2	..	
1823	460	..	..	..	
1824	456	2	3	..	
1825	420	3	5	..	
1826	403	2	1	..	
1827 <sup>a</sup>	..	..	..	..	<sup>a</sup> Returns for 1827 deficient.
1828	431	8	234 <sup>b</sup>	..	<sup>b</sup> These fevers were all returned under the head of "Autumnal Bilious Remittent Fevers," and latterly as "Malignant," being all of the prevailing epidemic.
1829	484	8	..	..	
1830	481	..	..	..	
1831	471	3	..	..	
1832	494	6	1	..	
1833	532	..	..	..	
1834	451	2	..	..	
1835	470	2	..	..	
1836	462	6	..	..	
1837	467	8	..	..	
1838	439	..	..	..	
1839	426	..	..	..	
1840	477	..	..	..	
1841	469	5	..	..	
1842	549	3	2	..	
1843	505	19	1	..	
1844	495	2	3	..	
1845	514	1	2	..	
1846	531	5	1	..	
1847	645	6	..	..	
1848	684	13	..	..	Very few of the cases of Intermittent Fever were indigenous; having been mostly relapses of disease contracted at other quarters.*
1849	723	23	..	..	
1850	764	1	..	..	
1851	725	..	..	..	

(Signed) J. STEWART,

Inspector General,

Ordnance Medical Department."

Woolwich, 17 February, 1852.

\* This opinion of Dr. Stewart is, no doubt, well founded; but it is a matter of universal experience that the tendency to relapse in ague is much greater in countries where that disease originates. Moreover we learn from the Royal Engineers Office, that works requiring a very considerable removal of earth had been commenced at Gibraltar during some of these years, especially in December 1842, and in the course of 1848-49. See p. 57.

remittent of the African coast, became, from a series of causes, exalted into a concentrated remittent or Yellow Fever, and in that manner acquired new and peculiar properties, not primarily and essentially belonging to it.

With reference to this latter opinion, it may be observed that the Governor-General of the Cape de Verd Islands affirms, that not one of those who with a view to escape the pestilence emigrated to the different islands of the Archipelago, had the disease, or communicated it to others. According to the view of Dr. McWilliam, therefore, this disease must have been of a very singular character, for in its origin at Shebar, it was not contagious, at Boa Vista it became contagious, while in the other islands of the Archipelago, wherever the sick or the uninfected fled, it again laid aside its contagious character, and did not spread to a single individual.

All the inquirers and reporters agree in stating that among the causes which concurred in communicating to this disease so extraordinary a degree of prevalence and mortality, the more important were the following:—

The employment of the crew uninterruptedly for an unusual length of time, including the sickly season, in a peculiarly unhealthy situation, and dangerous local duty.

The exposure of men, whose systems were impregnated with the seeds of disease imbibed in this unhealthy locality, to the risks of unrestricted liberty on shore, in the atmosphere of Sierra Leone, during the rainy season; one consequence of which freedom being their “inordinate indulgence in ardent spirits of the worst description.”

And subsequently, at Boa Vista, the confinement of the crew, the sick as well as the uninfected, in a place still more crowded, filthy, and unventilated than their quarters on board, instead of their dispersion in a pure atmosphere.

Some conception may be formed of the unfavourable circumstances under which the crew were placed at the Fort, from the account which, on personal inspection, Dr. King gives of its sanitary condition, who states that from the absence of all means of cleansing, from the actual accumulation of filth, and from the impossibility under any circumstances of obtaining a free circulation of pure air, owing to the plan of the building, the atmosphere which the sick, the convalescent, and the healthy were

compelled to breathe, day and night, must have been polluted and deleterious in the extreme; and that into a space incapable of affording sufficient accommodation for 50 men, upwards of 100, including the sick, were huddled together under a most oppressive heat, the thermometer ranging from 81° to 86°. This description is confirmed by the testimony of Dr. Almeida, who states that having been requested by the Governor-General to go to the Fort and see the sick, "he found them so extremely crowded that he could hardly pass between them."

The influence of such conditions in conducing to the virulence and spread of the disease has been already exemplified in what has been stated under the head "Localizing Causes;" but it must be added, that the crew had here also access to ardent spirits, in which both the sick and the uninfected indulged to still greater excess even than at Sierra Leone.

"It is with great regret," says Sir William Burnett, "I have now to state on the best information, that while in this situation means were found to supply the sick as well as others with enormous quantities of ardent spirits, which were drunk with avidity and produced the most deleterious effects; indeed I have reason to believe that some were absolutely killed by it as if by poison. Had there not been a fever already in existence, the intense heat (86° of Fahrenheit), the nature of the soil, and this dreadful intoxication together, would have been fully sufficient to have produced it, and one of the worst kind too, in which irritability of the stomach and dark-coloured vomiting would have been conspicuous symptoms."

The actual result, as stated by Dr. McWilliam, was that the accession to the sick-list and the mortality became much greater at this time than they had been at any previous period, and that from an endemic remittent of the African coast, the disease became exalted into a concentrated remittent or Yellow Fever.

Indubitable evidence further shows that, in addition to all these causes of disease, the crew when on board were constantly inhaling a poison generated in the ship itself. On a superficial examination the ship may have appeared clean, and Sir William Pym positively asserts that she was so; but there is conclusive evidence that this appearance was fallacious.

From the records of the Medical Department of the Navy we have extracted the following decisive statement with reference to this point, by Captain Simpson, late of the "Rolla:"—

"In June, 1845, being then in command of the 'Rolla,' I went on board the 'Eclair' off Shebar River. Commander Estcourt reported to me that he had sent a boat up the Sherborough River, and that the crew, during night, were exposed to heavy rain and much lightning, and were sick: some deaths had occurred on board. In the early part of July I went to Sierra Leone for supplies; the 'Eclair' was there; the vessel was anchored close to the shore; and I advised her Commander to move her further out, which he did. There seemed much excitement amongst the crew; some liberty had been given them, and drunkenness and sickness were the consequence. Wood was received on board for fuel in lieu of coals. This wood was green, as I understood at Sierra Leone, and very unhealthy to burn."

This fact is substantiated by the log of the "Eclair," which shews that from July 16th to the 19th inclusive, the crew were employed at Sierra Leone in wooding.

The influence of a quantity of green wood recently taken on board a ship navigating the tropical seas, in producing destructive fever, is shown in the most striking manner by the history of the "Regalia," already given (page 64), and by that of the "Vestal" (Appendix III., page 299).

Further evidence will be found in the Medical Department of the Navy to show "that the hold of the 'Eclair' was in a pestiferous state;" and Dr. King states, that long after the people left the ship in England, and when the engines were removed, mud, some inches deep, was found under the flooring.

"I should scarcely have noticed the above circumstance," he says, "but for some remarkable occurrences which took place in the same vessel at a subsequent period, which confirmed me in the opinion I had previously formed that the origin and continuance of the fever on board depended solely on local causes.

"The 'Rosamond,' formerly the 'Eclair,' was commissioned at Woolwich on the 5th of November, 1846, for the Cape of Good Hope station, but none of the former crew rejoined the ship. During the time of fitting out, four cases of typhus fever occurred, and were sent to the hospital, where two of them died, but it is necessary to mention that typhus was prevalent at Woolwich at the time. The steamer left England for the Cape on the 23rd of

February, 1847. Three days after sailing, one of the men was affected with slight febrile symptoms, and he continued more or less indisposed for a number of days, but occasionally felt so well that he returned to his work. After the ship entered the tropics, however, the disease began to assume a new and alarming character; and when off the Island of St. Nicholas, and almost in sight of Boa Vista, the man died, having had for two days previous black vomit and other characteristic symptoms of Yellow Fever. Within a few days afterwards the 'Rosamond' arrived at Ascension, where I was then stationed; and Commander Foot having communicated to Captain Hutton, the superintendent of the island, every particular respecting the illness and death of the seaman, I was ordered with Dr. Sloane, the surgeon of the hospital, to make a report on the case, and at the same time to suggest measures for the benefit of the ship without endangering the health of the people on the island. Having obtained from Dr. Slight, surgeon of the 'Rosamond,' every information relative to his late patient, we stated our opinion that the disease the man died from was sporadic Yellow Fever. \* \* \* On the following morning I went on board with the view of learning something to enable me to form an opinion as to the sanitary condition of the ship, and for the purpose also of inspecting the sick, as the surgeon informed me he had then a suspicious case, with symptoms of a low kind of fever. I had barely time to take a cursory view of the after parts of the ship, when my attention was called to the patients, who were all mustered in the steerage, and I found the man the doctor had alluded to in such a precarious state that I recommended him to be sent on shore immediately. The only other severe case was that of a supernumary lad, who was taken ill the same morning, but the indications of a low malignant fever were so apparent even at that early stage, as to induce me to express my opinion to the surgeon that he would not probably survive 24 hours. As it was most desirable to prevent a panic among the ship's company, I went on shore to consult with Captain Hutton, and make arrangements for their reception. \* \* \* The patients themselves attributed their illness to foul air in the forepart of the ship; one of them said he suffered so much from an abominable stench in the boatswain's store-room, that he represented the circumstance and obtained permission to cut a hole in the floor, which exposed to view a considerable quantity of soft mud, and five or six buckets full of it mixed with decayed shavings, and emitting an offensive odour, were removed at the time.

"It appears then, that besides an unusual number sleeping in the fore-cockpit, some of them at least had been exposed to a morbid miasma, exhaled from a festering mass of filth in the bottom of that part of the ship. The quantity of mud, no doubt, was small in comparison with what had accumulated when the vessel arrived at Spithead from the coast of Africa, yet the malaria eliminated

from that small and circumscribed focus was equally virulent in its operation, and produced the same disease in a few who were placed within the sphere of its influence."

Such is a brief narrative of the circumstances connected with this ship and her crew.

But it has been alleged that while the landing of the crew of the "Eclair," at Boa Vista, afforded no benefit to the ship's company, it inflicted a grievous evil on the inhabitants of the island; that several individuals in contact, or close proximity with the sick, became affected with the same kind of fever; that from these individuals the malady spread to others with whom they came in contact, and from these again to others, as from so many centres of contagion, until the disease became general over the island, thus affording a positive instance of the importation of epidemic disease. The alleged facts on which these representations rest are the following:—

It is stated, that during the occupancy of the Fort by the crew, there was a small Portuguese guard stationed there; that this guard was several times relieved; that at the time when the "Eclair" left the island, the guard consisted of one negro and two European soldiers; that within three days after the sailing of the "Eclair," both Europeans were attacked with fever similar to that from which the crew of the "Eclair" had suffered; that the negro soldier, who, with his comrade—the man sent from Boa Vista to nurse the two Europeans—on returning from the small island to Porto Sal Rey, had been—"as a matter of precaution"—"restricted for [‘about 8’ or] 17 days to the occupation of a small hut at the northern end" of the town, was afterwards attacked,—though not confined to bed until the day following his return to barracks; and that a woman (Anna Gallinha), who lived next door to this hut, was the first person who was attacked with fever in the town. It is further stated that a man (Pathi), who had been a labourer on board the "Eclair," was also attacked with fever, according to one account, on the day after the "Eclair" sailed; but according to another account, on the third day after that event.

Such are the alleged facts, and the only ones bearing

directly on the communication of a specific contagion by the crew of the "Eclair," collected by Dr. M'William by personal inquiries on the spot; and these, in his opinion, present a chain of evidence sufficient to establish a positive instance of the importation of epidemic disease.

With reference, however, to these inquiries, it has been already stated that they were not instituted until several months after the departure of the "Eclair" from Boa Vista;—the only regular practitioner on the island (Dr. Kenny) who could have given authentic and trustworthy information respecting the nature and progress of the disease, had died;—the witnesses examined by Dr. M'William, poor and ignorant, gave their evidence, hearsay and otherwise, in the loosest possible manner;—their statements as to dates and occurrences, alleged to have happened several months before the inquiry took place, were received implicitly, without examination into the correctness of their answers and the credibility of their testimony;—all the witnesses of this class appear to have spoken under the influence of the strongest feeling of self interest with a view to establish a claim to pecuniary compensation should they be able to make out a case against the "Eclair," in which expectation they were not disappointed, since the sum of 1000*l.* was eventually granted by Great Britain for the benefit of the inhabitants;—and to this motive may probably be ascribed the highly coloured and exaggerated statements put forth by these people on the reappearance of fever in the following year.

Taking the facts, however, precisely as they are represented in the Report of Dr. M'William, we submit that they do not, as the proof of the allegation in question requires, present a clear and palpable chain of evidence, connecting as cause and effect the fever of the ship with the epidemic on shore; but on the contrary, that there is not a single link undoubtedly connecting the one with the other.

Take the first case forming what is represented as the first link in this presumed chain, the seizure with fever of the two guards at the Fort.\* Two European

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\* See Note, Appendix III., p. 309.

soldiers lately arrived in the colony, and therefore peculiarly predisposed to an attack of endemic fever, go from Boa Vista, which at that time was healthy, to a confined, unventilated, overcrowded, and filthy spot on another island where fever was raging to such a degree that within the space of three weeks there had occurred no less than 60 attacks and 33 deaths, in a crew consisting on the arrival of the ship of 117 officers and men. We submit that there is in this no evidence of the propagation of disease by a specific contagion; on the contrary, it is the ordinary production of disease by its ordinary cause, namely, exposure to a polluted atmosphere, the pollution being, in this instance, excessive from overcrowding; from accumulation of filth; from foul and offensive privies; from the impossibility of the admission of fresh air, owing to the construction of the building, and from the intense and oppressive heat, the thermometer ranging from 81° to 86° of Fahrenheit. The seizure of two men with fever under such circumstances is precisely analogous to the case to which we have so often directed attention—the attack of persons, previously healthy, with typhus, who take up their abode in the crowded and filthy courts and alleys of English towns.

Take the next link in the chain, the attack of the negro soldier. The circumstances respecting this man, being precisely the same as those relating to the two other guards, the same answer would have sufficed for both, but we may observe further, that according to the testimony of the man himself, his illness was very slight, and his companion who was sent to lodge with him at the hut in Porto Sal Rey, had no illness at all during the whole time of their seclusion.

The third link in the chain is the presumed fact, that a woman (Anna Gallinha), who lived next door to the hut in which these two men had been confined, was seized with fever soon after they had left it, and that she was the first person attacked, at least whose illness attracted public attention, in the town of Porto Sal Rey. Dr. King states, that on a personal examination of the soldier who had experienced the slight attack of fever, he said that during the seventeen days that he and his companion were confined to the hut, “they had no communication

with any one." Dr. M'William, on the other hand, affirms that Gallinha was a frequent visitor at the hut, and, indeed, cooked for the men. Supposing Dr. M'William's account to be the correct one, it is surely more reasonable to attribute the attack of Gallinha to the local causes to which she was exposed, and which Dr. M'William admits were sufficient to account for her illness, than to contagion derived from a man whose illness was so slight that it had not confined him to his bed for a single day, and which was incapable of infecting his companion who was constantly with him night and day.

"By the time Anna Gallinha was taken ill," says Dr. M'William, "much rain had fallen; the weather had become more hot, and, in short, there now (but not before this) existed the recognized elements for malarious evolution."

"In that part of the town called Beira, or Pao de Varella," reports Dr. King, "where Anna Gallinha and the soldiers resided, the houses are of the lowest description, and the people who occupy them are generally very poor and destitute; there is a large pool of stagnant salt and fresh water immediately behind; but to windward of this part of the town, and still nearer to the houses, there is a locality which is resorted to by many of the people when obeying the calls of nature; and the exhalations from the one, and the effluvia from the other, are blown by the north winds in the direction of Beira."

A similar description of this locality is given by Dr. M'William,—

"In the upper portion of the town," he says, "which is called Pao de Varella, the houses are in general mere hovels, rudely built and much crowded together, and with few exceptions dirty. They are occupied by the lowest classes. From the total absence of any police laws the streets here are also very filthy."

Here then were present in full force, as is admitted, the ordinary localizing causes of fever; to which, we submit, it is more consistent to refer this case, than to an extraordinary and foreign cause.

But at this point the presumed chain of evidence stops, the chain is suddenly snapped; there is no further link traceable; there is nothing really connecting the illness of Gallinha with the next cases, or with the general spread of the disease which rapidly followed, and we need hardly state, that in order to prove the spread of a pestilence by contagion, communication, either direct or indirect,

must be proved to have existed between all the persons attacked.\*

For the only other case of fever that is stated to have occurred shortly after the sailing of the "Eclair," namely, that of the labourer (Pathi) who had been employed on board the ship, will scarcely be considered as affording an additional link; since admitting that this man contracted his fever while employed on board the "Eclair," his case would be merely one of infection from going on board a foul ship, a generally recognized cause of fever.—

"Whenever," says Dr. Stewart, "fever has prevailed much in ships on the West India and African stations, strangers going on board of those ships have been particularly liable to its attack; but on sending fever cases from those ships to the hospitals and private houses on shore, it has not been found that the disease extended from them."

But as in the locality of the dwelling of Gallinha, so in the district in which this man lived, there were local causes abundantly sufficient to account for the endemic origin of his disease. He resided in Rabil, one of the hamlets in the neighbourhood of Moradinha, at some distance from Porto Sal Rey. Of this locality Dr. King says:—

"If there is one spot more than another in the whole island where, from its physical peculiarities, endemic fever might be expected to begin first, and end last, that locality is Moradinha, and the villages in its vicinity, in one of which Pathi resided."

It may be observed further that whatever may have been the cause of this man's fever, it is admitted, that for three weeks at least it was communicated to no one else in the house at Moradinha, where he was attacked, and remained for eight days, and not to any one else in that neighbourhood for 11 weeks; that his illness was extremely slight, and that on his return to his own house no disease broke out for some time in his family. According to Dr. M'William, the first member of his family that was attacked was one of his children, who was taken ill "on the tenth or eleventh day" after his return,

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\* The widow of the next victim (Affonso) denied his having had communication with Gallinha; and Dr. Almeida "found about 20 people sick" in Porto Sal Rey only 3 or 4 days after Gallinha's death. It is evidently more rational to ascribe these numerous attacks to epidemic influence, which it is admitted was now present, than to contact with this woman, for the fact of which there is in truth not a shadow of evidence.

the illness of this child being gradually followed by that of two other children. But Dr. King affirms that these children were not taken ill until "about a month" after their father's return, and that it was not until the succeeding month (the middle of November) that his wife was seized, "when the disease was general throughout the island." It is also particularly to be observed, that a child in another family at Rabil, having no communication with the family of Pathi, died about the same time as Pathi's first child, and that the disease broke out at least as early at Rabil as at Porto Sal Rey.

Lastly, it may be urged in opposition to the opinion that the contagion was communicated by the crew of the "Eclair," that the small island on which the sick were landed and to which they were confined was a mile distant from the town of Porto Sal Rey, and that on reference to the map attached to Dr. M'William's report, it is obvious that the North-east trade wind must (according to the theory of Sir William Pym, as applied to the Neutral Ground at Gibraltar in 1828, See Note, Appendix No. I., p. 169) have dispersed the contagion if in existence, or carried it in a contrary direction from Porto Sal Rey.

For a more minute examination of the cases of the guards at the Fort, and of Pathi and others, as presented by Dr. M'William, we refer to the Note of Dr. Browne, Appendix No. III. (p. 306), who has there shown the real value of these cases, considered as links forming a chain of circumstantial evidence.

The authentic facts attending the intercourse of the ship's company with the inhabitants of the island, afford further evidence that no infection could have been communicated by the former to the latter. Thus, it is admitted that Captain Estcourt, the commander of the ship, went directly from the infected vessel to reside with Mr. Macaulay, the judge: no infection was communicated to Mr. Macaulay, or any part of his family.

The officers of the gun-room—midshipmen, warrant, and engineer—on disembarking from the ship, took a house for themselves and their servants in the town, and mixed unreservedly with the inhabitants: no infection was

communicated to any individual with whom they had intercourse.

The crew obtained, or took leave to pay frequent visits from the small island to the town of Porto Sal Rey, where, according to Dr. M'William, they resorted chiefly to the house of one Georgio, who kept a spirit store; the only consequence of which visit, considered by Dr. M'William a remarkable one, appears to have been that this man (and "shortly afterwards" two females who associated with them) was attacked with headache and general fever on the evening of the day he was visited by the "Eclair's" people; a result which admits of a more obvious solution than the communication of febrile contagion on the part of persons who were themselves in perfect health.

The soiled linen of the officers and crew having been brought on shore on the first arrival of the vessel, was immediately given out to be washed to the washerwomen of Porto Sal Rey, and the careful search made after these women, brought to light no fewer than seventeen persons who were so employed.

"The soiled clothes," says Dr. King, "linen, cotton, and flannel, which had accumulated in the officers' cabin from the time of their departure from Sierra Leone, were contained in at least 12 bags, which were taken on shore at Porto Sal Rey the same evening the ship arrived, and distributed next morning (22nd August) to the washerwomen of the town. Now, if the disease possesses the power of reproduction, its poison must [according to general opinion] have been as certainly communicated through the medium of *fomites* as by direct contact with the sick on board or at the fort; yet none of the washerwomen nor any in their families were attacked with fever until November, showing an interval of 70 days after exposure to the infection."

That it was not from any want of susceptibility to the influence of febrile poison that these women escaped the danger of this exposure to *fomites* was proved by subsequent events; for during the progress of the epidemic, all of these women, according to Dr. M'William, with only one exception, were attacked with the prevailing fever; two between six and seven weeks after the sailing of the "Eclair;" five, two months; two, three months; three, four months; and one, five months afterwards.

"None of the deaths," says Dr. M'William, "took place until fever was general in Porto Sal Rey, so that in none of these cases can the occurrence of the fever be fairly attributed to infectious matter conveyed by the linen."

The Guards at the Fort were many times relieved, and the soldiers were sent direct from the small island to their barracks in Porto Sal Rey, without conveying any disease to their comrades. On one occasion two soldiers who are stated to have lived in a room next to that in which the sick of the "Eclair" were lodged, on being taken ill, were conveyed at once to the barracks, yet they infected no one in their quarters.

From a list drawn up by Dr. King, of the names of the islanders who were engaged as labourers on board the "Eclair," it appears that there were in all 63 persons employed in coaling, watering, and cleansing the ship. These men appear to have had unrestricted communication with the ship's crew. According to Dr. M'William, the whole of these labourers went to their respective homes every night, except those from Estacia and the Eastern villages, who generally slept at Porto Sal Rey. None of these men were themselves attacked with fever, excepting one (Pathi) whose case has been already considered; none of them communicated fever either to their own families or to the persons with whom they lodged in the town, yet subsequent events proved that they as well as the washerwomen were sufficiently susceptible subjects, since, during the progress of the epidemic, the greater part of them were attacked by the disease; none, however, within a month after the departure of the "Eclair;" a few within two months, but the majority not until four or five months afterwards.

That the geographical position of the Cape de Verd Islands places them within the legitimate domain of Yellow Fever, and that this disease is no stranger to these islands, is admitted on all hands. According to Dr. M'William,

"The north-western part of the island where Porto Sal Rey is situated, is low and flat, and almost wholly occupied by sand, which, blown up from the north-western shore through the water-courses, and other hollows, accumulates in mounds twenty and thirty feet high, which are drawn about and shifted by any little variation of the direction of the wind."

On the flat between Porto Sal Rey and the village of Rabil, which is about four miles to the southward of Porto Sal Rey, Dr. M'William states, that there is a point where the sea, when the waves are high—

“Breaks over the elevated beach, and penetrates through the shingle, so as to accumulate, and run inland in the form of a narrow creek, from 200 to 300 yards from the sea shore. During the rainy season, this, in common with the other flats on the island, is inundated to a considerable extent, as is evident from the appearance of the soil in those places not covered with sand, as well as by the presence of a rude raised causeway, which the people have constructed over part of the hollow flat, to render it passable during the rains.” \* \* \* “Near the town is a hollow flat, spread over an area of about a mile, with the same soil and subsoil as that in the town. The central part of this area is occupied by a salt pan, which contains not less than 300 troughs, each a foot deep, and about thirty feet square, into which the salt water is poured, there to evaporate and form salt. During and for some weeks after the rainy season, the whole of this space is more or less inundated. \* \* \* “The water is left to stagnate on the Rabil side, and as it dries up during the hot weather, little alluvial islets are from time to time exposed, which the people avail themselves of to raise a small crop of corn. Indeed the greater part of the ravine, from Rabil downwards, is in a state of rude cultivation, and contains large green foetid pools, with all kinds of decomposing matter, the effluvia from which was most offensive when I was there in May, 1846.”

Experience has shown, that such a condition of sandy soil is as fruitful a source of endemic and malignant fever as a marsh or swamp. Dr. Lind, who wrote nearly a century ago, expressly notices the unhealthiness of Boa Vista, particularly during the rainy season, stating that, “strangers who arrive here at this season are liable to be visited by a general sickness,” and instances its white sand as a mark of an unhealthy locality. Dr. Fergusson confirms the correctness of this indication of insalubrity.

“That sandy soils,” he says, “should, in malarious climates, prove as productive of aggravated remittent fever as the swamp, has never been sufficiently explained. Certain it is, however, that they do so, in a marked and prominent degree. The Alemtejo and Algarve of Portugal—regions, I may say, altogether of sand—are the most prolific of fever of any in the Peninsula.”

Another instance is found in the unhealthiness of

Vera Cruz, which is spoken of by Mc Culloch in the following words:—

“It is said to be the original seat of the Yellow Fever.” [Bulama?] “The city is well built and the streets clean, but it is surrounded by sand-hills and ponds of stagnant water, which, within the tropics, are quite enough to generate disease. The inhabitants and those accustomed to the climate are not subject to this formidable disease; but all strangers, even those from the Havanah and the West India Islands are liable to the infection. No precautions can prevent its attack, and many have died at Xalapa, on the road to Mexico, who merely passed through this pestilential spot.”

Dr. King states, that if ever endemic fever derives its origin from a vitiated and malarious state of the atmosphere, Boa Vista abounds with the elements for its production. Among these he enumerates swamps and pools of stagnant water, in the immediate vicinity of Porto Sal Rey, and over the whole district of Rabil; patches of rich alluvial soil near the other villages, the recognized sources of noxious exhalations; the wretched food of the lower classes, and still more, the polluted atmosphere which they breathe in their crowded and ill-ventilated abodes, and the general disregard of cleanliness in their houses and streets, “a combination of morbid causes,” he says, “which would produce malignant fevers in any part of the world.”

The relative position of Boa Vista to the African coast would further naturally lead to the expectation that it must be subject to diseases of the same character, and no one disputes that this is the case. The residents of the island, military, medical, and civil, concur in stating that endemic, bilious remittent fever, prevails there more or less every year; that there is no season in which it does not carry off several of the inhabitants, and that it often prevails epidemically.

“The testimony of the most intelligent men in the island,” says Dr. King, “including Dr. Almeida, Senor Baptista (the Consul’s agent), the Mayor of Rabil, the Judge of Fundas Figieras, and the Judge at Old Town, removes every doubt as to the fact that fever prevails to a certain extent, and carries off several of the inhabitants in the months of November and December every year; and this endemic fever, which recurs annually, and which Dr. Almeida calls the bilious remittent, does not always present the same mild aspect and character;

on the contrary, it is well known that in certain years the disease was epidemical, and in comparison with other seasons, very fatal."

Dr. M'William records the fact, that such epidemic seasons occurred and proved unusually mortal in the years 1821-2, in 1827, and in 1833.

It is most material to a right understanding of this whole subject to observe, that a Yellow Fever Epidemic had broken out at this very time in an adjoining island, St. Jago. It is stated by Dr. Stewart, in his Report in the Admiralty Correspondence, that "in the adjoining island at Porto Praya, there was Yellow Fever whilst the ship was at Boa Vista." Captain Simpson states that it recurred in the following year at Porto Praya; "is common there at times and quite endemic."

That co-incident with the presence of the "Eclair" at Boa Vista one of these epidemic seasons was impending, was declared by the usual indications, which in warm climates precede and accompany such visitations. These premonitory signs on this occasion were a great fall of rain at an unaccustomed season; the consequent accumulation of large quantities of stagnant water in and about the towns and villages; the occurrence of extraordinary heat; the prevalence of light winds with frequent calms rendering the weather extremely sultry and oppressive; the appearance of sporadic cases of fever of more than common intensity; the almost simultaneous outbreak of pestilence amongst cattle and other domestic animals; and the visitation in greater numbers than common of destructive insects.

These prognostications were so manifest as to excite the attention and alarm of the intelligent classes of residents. The Governor-General states:—

"Great falls of rain took place at a very advanced period of the season, which remained stagnant."

The British Consul says:—

"Up to the month of October, extraordinary heat and the fall of a large quantity of rain had been experienced, events which were surprising to the oldest inhabitants."

The British Judge says:—

"Stagnant water had settled in great quantity at the back of the town, to which was joined great heat in the weather.

Dr. King says :—

“The information received on the island in 1846, fully corroborated what is stated in the above extracts, the periodical rains, contrary to what usually happens, did not set in till late in September. In October, November, and December the winds were light and variable, with frequent calms, and the weather became in consequence extremely sultry and oppressive. The grass and green crops were nearly destroyed by the long previous drought, and what little appeared after the rains was devoured by the locusts which visited the island in greater numbers this year than was ever known to be the case before.”

Though Dr. M<sup>c</sup>William, on his inspection of the island with a view to ascertain the true cause of the pestilence, took no notice of any of these premonitory signs of its approach, Sir William Burnett was fully aware of their signification, and calls special attention to one of the most important of them in his Report to the Lords of the Admiralty.

“I beg to lay before their Lordships,” he says, “an extract of a letter from the Governor-General of the Cape de Verd Islands, and likewise extracts of letters from Mr. Macaulay and the British Consul, residents on the island of Boa Vista, distinctly showing the very remarkable state of the weather preceding the attack of the inhabitants of the island, which very important circumstance in a case of this kind, I regret to observe Dr. M<sup>c</sup>William has omitted to take any particular notice of.”

The event foreshadowed by these occurrences rapidly followed. As early as the middle of September a few cases of unusually malignant fever broke out, but as has been already stated, the first case that attracted public attention occurred on the 12th of October; a few others followed during the remainder of this month; a still greater number broke out in the beginning of November, and the epidemic came to its height in the latter half of November, continuing to prevail throughout December, and recurring for several months in the following year.

As in epidemic outbreaks in general, so in this instance, individual or sporadic cases occurred some time before the appearance of the epidemic in its true and proper form. On minute inquiry, it was discovered that one if not two cases occurred as early as the 14th of September (Pathi), another on the 20th of September (Roque), and a third on the 21st of September (Agostinho): no other cases,

at least none that attracted attention, appear to have occurred until the one already mentioned (Gallinha), on the 12th of October. These sporadic cases all occurred in the ordinary localities of epidemic disease, and among individuals belonging to the classes that usually furnish its first and chief victims.

Events precisely similar preceded the outbreak of the Yellow Fever epidemic which is prevailing at the present time in George Town, Demerara. By dispatches from British Guiana, dated the 6th of January, 1852, it appears, that although the disease did not assume an epidemic form until about the 30th of December, 1851, yet that individual cases of a highly malignant character occurred as early as the beginning of November; and that concurrently with these sporadic cases a marked change was observable in the type of the ordinary fever of the colony.

"I have observed," says Dr. Blair, in a letter addressed to Governor Barkley, and forwarded by His Excellency to Earl Grey, "that many cases of what appeared the ordinary fever of the colony seemed modified and exasperated, as if some new visitant had been added to the endemic malaria. Indeed, as early as the 7th and 24th of October a slight change in the usual manifestation of fever symptoms might have been observed in a few cases. But from the beginning of November the change became marked. Unusual flushing of the face, vascularity of the eyes, trifling hæmorrhage, supra-orbital headaches, and epigastric oppression supervened as a first or second paroxysm of fever, and such cases were intercurrent with nominal intermittent. In one or two instances sudden and unexpected death occurred.

"The evidence of a new virus (new because unknown for several years) poisoning the atmosphere, continued to accumulate, and its specific character has gradually developed itself until it has become too manifest that we are again invaded by a similar epidemic to that of 1837."

Dr. Blair further states that the epidemic commencing with "a very slight change in the character of the ordinary fever," continued to manifest its presence and progress by an increase in the intensity of the symptoms, and by the addition of new ones, which successively approximated it more and more closely to decided Yellow Fever, until at length, about the 30th of December, 1851, "it assumed the perfect signs of the most virulent type of the disease."

In a despatch, dated George Town, 27th January, 1852, giving an account of the progress of this epidemic, Dr. Gavin states, that the present outbreak shows that Yellow Fever, like Cholera, returns not only to precisely the same localities, but even to the same houses; and that immunity from the disease may be, to a great extent, guaranteed by the avoidance of the infected localities.

"Hitherto," he says, "under this instruction [avoidance of infected localities], the white troops have remained perfectly free from disease. Nothing can more clearly mark the influence of localizing causes in the development of this disease than the facts in relation to the present epidemic. The parties upon whom the mortality has almost without an exception fallen, have been those who have been living, or who have remained for a determinate period in the foulest localities of the town, and by the river side—localities notoriously known as sinks of filth and impurity [*see p. 45*]. The disease has fallen almost entirely on the European sailors, in certain moorings, and on the Portuguese, who live in the greatest filth, and whose shops and premises abound in the most offensive impurities. Comparatively few English inhabitants have been attacked, and those who have, dwelt in the very midst of the localities proved by experience to be the most offensive and dangerous, in such circumstances as the present, to newly-arrived Europeans."

We have already adverted to the testimony that at Boa Vista, in addition to other proofs of the presence of a stagnant and pestilential atmosphere, there was the evidence derived from the prevalence of unusual sickness and mortality among domestic animals.

"That the common air," says Dr. King, "which was inhaled by every living thing on the island was in an epidemic condition in the months of October, November, and December of both years, is sufficiently demonstrated by the simultaneous occurrence of universal sickness and great mortality among the cattle (including horses, cows, mules, donkeys, and goats) at the very time that fever was raging among the inhabitants. And, further, there was this remarkable coincidence, that after an interval of some months and the disappearance of the disease both in man and beast, the same fever broke out again in the towns and villages about the rainy season of the following year, and was again accompanied by the same murrain among the cattle, which in the two seasons proved fatal to two-thirds of the whole stock of the island."

We submit that these considerations afford all the

evidence which the nature of the case admits of, that the sickness which affected the island on this occasion arose, not from the landing of the sick of the "Eclair," but from climatic and endemic causes.

To sum up the whole of this case, then, it appears that the evidence in favour of the allegation that fever was imported into Boa Vista by the "Eclair," amounts to this: that four men, not of the ship's crew, were attacked with fever while performing military service in a locality in which no fewer than 60 of the crew themselves were seized; that one man not of the ship's crew who worked as a labourer on board the ship "about eight" or "two" days, had a slight attack of fever, while 62 men also not of the ship's crew, and who also in like manner worked as labourers on board the ship a longer time, were wholly unaffected; and that a month after the sailing of the vessel, a woman was attacked with fever who happened to be a next door neighbour to two of the soldiers who had served on duty at the Fort—one of whom was unaffected, and the other not even confined to bed—simultaneously with the children of the labourer (Pathi) who resided in one of the dirtiest localities of the island.

Against such evidence, if evidence it can be called, must be weighed the following countervailing considerations:—

It is admitted that the "Eclair" had been exposed on the coast of Africa to the causes which usually develop epidemic fever in that country; that intensity was given to those causes by circumstances which occurred at Sierra Leone, where she took in green wood as fuel, and where her men went on shore during the rainy and sickly season, and indulged in the unlimited use of ardent spirits; that her hold was in a pestiferous condition, and that a quantity of putrid mud had collected between her timbers. It is proved that the fever which broke out under these circumstances was the common endemic African coast fever, which, it is admitted, is not contagious, and which is assumed to have become contagious on this particular occasion, expressly to account for its alleged importation. It is admitted that on the landing of the ship's crew at Boa Vista, though the men mixed freely with the islanders,—though the officers lodged in the town,—and though when some of them became sick,

they were nursed by the inhabitants,—there was no communication of the disease in a single instance. It is admitted that of seventeen washerwomen who washed the linen of the officers and crew, not one became infected, although all these women, except two, suffered severely from the disease at subsequent periods after the epidemic became general. It is admitted that with the exception of one case, which has been proved on inquiry to have been no real exception, 87\* labourers worked on board or in the neighbourhood of the ship daily, and returned to their homes at night, without taking any precautions,—without becoming themselves infected,—and without communicating infection to any individual of their families ;—though, like the washerwomen, the greater part of these men suffered severely when the epidemic became general. It is admitted that the Cape de Verde Islands are within the Yellow Fever zone, and are liable to frequent and severe outbreaks of epidemic fever. It is admitted that the physical and social conditions of Boa Vista are eminently those which are found by universal experience to localize epidemic diseases whenever an epidemic influence is present. It is admitted that the “Eclair” arrived at Boa Vista at the season of the year when endemic fevers usually prevail. It is admitted that at the very time of her arrival, Yellow Fever was actually prevailing at Porto Praya, in the island of St. Jago, into which it is not alleged that the disease had been introduced by importation. It is admitted that some time before the outbreak of the epidemic, the atmospheric and other conditions which usually precede and accompany the development of epidemic disease, were so manifest as to attract general attention. It is proved that sporadic cases of the disease appeared, as is usual, some time before the presence of the epidemic was declared in its distinct and recognized form. It is admitted that the epidemic influence extended to animals as well as man, a mortal epizootic disease prevailing over the whole of the island at the same time. It is proved that the epidemic did not break out until about a month or six weeks after the “Eclair,” with all her crew, healthy and sick, had left the island.

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\* The aggregate number of the lists furnished by Dr. M<sup>c</sup>William.

It is admitted that a similar epidemic appeared among men and animals the following year, not imported, but entirely of local origin.

A consideration of these circumstances has satisfied most of those who have inquired into the case, that the arrival of the "Eclair" at Boa Vista with fever among her crew, and the occurrence of a similar disease on the island, were mere coincident events, and that the appearances which might at first view have given some colour to the notion of importation were fallacious.

Among those who arrived at these conclusions were—The Governor-General, who says:—

"The disease was perfectly endemic, not one of those who emigrated to the different islands of the Archipelago had the disease or communicated it to others. It did not make its appearance till a month after the departure of the steamer. . . . The disease had its origin in the great falls of rain which took place at a very advanced period of the season, and which remained stagnant in the neighbourhood of the place."

Mr. Rendall, the Consul, who says:—

"The competent officers of the 'Eclair' at all times pleaded that the fever which had appeared and rested on board was nothing more than the 'common African coast fever;' the opinion of the medical men on the spot continued to be that the fever was merely the common African fever, and that no danger existed of its spreading among the people."

Mr. Macaulay, the Judge, who says:—

"So long an interval had elapsed between the departure of the 'Eclair' and the appearance of the first serious case of fever in the town, that we were all disposed in the first instance to attribute it, as well as the general sickness of the place, rather to stagnant water, which had settled in great quantity at the back of the town, joined with the great heat of the weather and the dirty state of the streets. The 'Eclair' had left Boa Vista nearly a month before any case of fever exhibited itself in the town. . . . No injury whatever had resulted from the unrestricted intercourse which had subsisted during the whole of the 'Eclair's' stay in the harbour, between the officers and men (not in hospital at the fort) and their friends on shore."

Captain Simpson, who says—

"If I give my opinion on the fever that was on board the 'Eclair,' I should say it commenced at Shebar: and it was to be expected that men being exposed in boats to night duty during the rains, would be sickly; that it was likely to be much increased

at Sierra Leone by the long continuance of the vessel there, and the men having leave to go on shore during this season, when this place is so very unhealthy, and seamen always so incautious; the occupation of the 'Eclair's' officers and ship's company on board the 'Albert' in clearing the holds, at all times a very dangerous work in the Tropics; and the use of green wood for fuel. In fact, I should have been very much surprised if the 'Eclair' had not been sickly."

Sir William Burnett, who, in reporting on the case to the Lords of the Admiralty, says:—

"After a careful perusal of the papers he (Dr M<sup>c</sup>William) has sent, I am compelled to say that I cannot conscientiously arrive at the conclusion the Doctor has done, namely, that the fever was occasioned by intercourse with the 'Eclair.'"

Sir William Burnett adds, with reference to the general question of importation;—

"With respect to the importation of the disease into various places, except in one instance, and that even is surrounded with doubts (I mean that of Her Majesty's sloop 'Bann'), I entirely disbelieve it. Both the surgeons of Bermuda Hospital most distinctly deny on two occasions that the epidemic which prevailed in 1843 was imported or contagious; I have also caused the medical reports of Jamaica Hospital for more than twenty years to be examined; and though hundreds of patients with yellow fever in all its most appalling forms, including black vomit, &c., have been treated in that establishment, not one of the medical officers in charge of the hospital have ever hinted at the disease being contagious; and if it be needful I can cite numerous other instances."

As to the apprehension that the crew of the "Eclair" might have imported the disease into England, he says:—

"I have no hesitation in declaring my firm belief that the sick men of the 'Eclair,' when that ship arrived at the Mother-bank, might have been landed at Haslar Hospital and placed in the well-ventilated wards of that establishment without the public health suffering in the smallest degree. It is a fact well known, and of the truth of which I can give the most satisfactory proof, that during the autumn of every year merchant-ships arrive in our harbours loaded with the produce of the coast of Africa, having perhaps lost great part, nay in some instances the whole, of their crew by the fever of the country; or some are still labouring under fever when the ship arrives in the Thames, and are sent to the hospital in that state; yet no instance is known of any infection having been produced by such procedure; in fact it is perfectly certain that it never did take place."

Dr. King, who says:—

"The inhabitants in general are firmly persuaded that the fever was imported by the 'Eclair' and afterwards spread throughout the island by contagion from one person to another. I have taken considerable pains to trace out and discover the supposed morbid concatenation, but in vain. It becomes, therefore, a duty to express my opinion decidedly, that there is no satisfactory proof of the disease having been propagated by contagion, or from a specific poison which is said to emanate from the bodies of the sick, the dying, or the dead."

We have entered into this particular examination of the case of the "Eclair" because as has been already stated, it is the one on which the greatest reliance is placed in proof of the importation of epidemic disease.

On the outbreak of the Yellow Fever epidemic of 1847, at Barbados, it was affirmed as usual, that this was another case of importation; but the real facts are thus stated by Dr. John Davy:—

"An opinion, I am informed, has been expressed that the disease was imported, and by Her Majesty's war-steamer 'Growler,' from the coast of Africa. That ship was employed in conveying liberated Africans to the West Indies. She left Sierra Leone on the 12th of November, 1847, and arrived at Trinidad on the 5th of December. During the voyage 46 deaths occurred amongst the emigrants from chronic dysentery, and two deaths amongst the crew from fever. The emigrants were landed at Port of Spain. No fever was there introduced, although no precautions were taken that I have been able to learn to prevent, unrestricted communication. She took her departure from Trinidad on the 8th of December; arrived at Barbados on the 10th; proceeded from thence on the 18th, and reached the Bermudas on the 24th. There, according to established usage, having come from the coast of Africa with fever on board, she was placed in quarantine: the crew landed and kept apart—the sick from the well—and the vessel thoroughly cleaned and fumigated. On examining the holds and bilges, they were found in a very offensive state, from accumulated vegetable matter, rice, chips, and shavings, in a decomposing state. The atmosphere in the carpenter's store-room would not support combustion. The fever was not communicated at the Bermudas to a family of five persons associated with the sick on shore; but two individuals from the shore, who were engaged on board in conducting the cleaning of the vessel contracted it, as did also a certain number of the crew similarly

employed, and only those so employed. The total number of cases of fever under treatment from the 'Growler,' were 75, of which three only terminated in death, 72 having recovered. These particulars I have collected from an extract from the Journal of Mr. Robert M'Crac, Surgeon of Her Majesty's ship 'Growler,' a copy of which interesting document is now before me, with which I have been favoured by the head of the medical department of the navy, Sir William Burnett."\*

At the recent Sanitary Conference at Paris, particular stress was laid by one of the delegates, M. Grande, on the case of the "Tentadora" and "Duarte," which vessels, he alleged, had very lately introduced Yellow Fever into Oporto. It appears that these vessels arrived at Oporto from Brazil during the autumn of last year, that cases of Yellow Fever occurred on board during their passage, and that several persons who visited the ships at Oporto were attacked with Yellow Fever. It was argued by Dr. Sutherland that these facts, supposing them to be correct, afford no proof of importation; that it was only those individuals who actually went on board the ships who were attacked; that none of those who were attacked communicated the disease to any person in the town, though no precautions were taken to isolate the sick; that this case offers an additional example of the fact that it is the ship which is to be feared, and not the country from which she arrives; that, consequently, it was absurd to put arrivals from Brazil under quarantine; and that the only rational means to be adopted in the present instance was to take immediate measures for cleansing the infected vessels.

Had it happened by any coincidence that an outbreak of fever should have taken place at Oporto within any reasonable time after the arrival of these ships, it would have been asserted, on so-called incontestible evidence, that Yellow Fever had been imported into this town, as was affirmed in the case of the "Eclair" at Boa Vista.

We deem it needful to advert only to one instance more of alleged importation; namely, the introduction of the Yellow Fever epidemic of 1828 into the garrison of Gibraltar by the ship "Dydden." We beg to call special atten-

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\* See Note, Appendix I., p. 227.

tion to this case, because it has been more rigorously examined than any other, and because on that account it exhibits a better specimen than can usually be obtained of the manner in which the evidence for these cases is commonly got up.

The most positive assertions having been made that this epidemic was introduced into Gibraltar by a ship from the Havannah, the "Dygden," the then Secretary of State for the Colonies, Sir George Murray, appointed a Special Commission to inquire into the facts of the case; consisting of the Judge Advocate, the Colonial or Civil Secretary, the Captain of the Port, and head of the quarantine department, the Town Major, or head of the police, the Principal Medical Officer of the garrison, and a Staff Surgeon. It was the desire of Sir George Murray that the Governor should act as president, on the ground that "as the proposed investigation is merely to ascertain a fact, it may be more properly accomplished by the careful examination of impartial witnesses than by the application of scientific research;" but Sir George Don, "not finding himself equal to the task," appointed, by desire of the Secretary of State, conveyed in a subsequent dispatch, the British Superintendant of Quarantine, Sir William Pym, to preside in his place.

The facts alleged and attempted to be established before the Board with a view to prove that this epidemic was imported by the ship "Dygden" were, that this ship had arrived from the Havannah with Yellow Fever on board; that while in quarantine in the bay, she was visited from the garrison by a family of the name of Fenic, and that the first cases of the epidemic occurred in this family.

The first witness called to prove this alleged visit to the ship was a woman of the name of Villalunga, who stated that she lived in the yard of Fenic's house; that Fenic was a cigar maker, that she assisted him in making cigars, that she heard the boy (Fenic's son) say that he, his sister and his father had been on board the ship in the bay on Sunday, the day before the boy was taken ill, and that the boy told her that they had been on board "to eat, drink, and make merry," and "that his father had sold tobacco on board the ship."

The next witness brought forward was a child Caffiero, 11 years old, who stated that he was in the habit of playing with the two Fenics: that he lived very near them; that he played with them *every day before their death*, and that he saw them *every day* when they were sick in bed.

On these statements the Judge Advocate, Mr. Howell, observes:—

"The only evidence which up to this period (April 10th) had been given to connect the illness in Fenic's family with a visit on ship-board, is the hearsay tale told by Villalunga, nor did she give to Fenic and his two children any companion in their alleged Sunday excursion." \* \* \*

"Eight days after his examination above mentioned, the boy Caffiero re-appears as a witness (viz., April 18th) with a story entirely new, and which, if credible, would be extremely material; because he affects to speak of facts which had before rested on the hearsay evidence of Villalunga, but of which facts Caffiero now, after the lapse of eight days, represents himself to have been an eye-witness. On this his re-appearance, however, he carefully abstains from giving any date, either day of the week, or month, or even season of the year. This cautious avoiding of dates may not unfairly be attributed to the variances between himself and Villalunga, in their respective journals of the illness of Fenic's children. Caffiero now says, 'I knew Salvo and Catalina Fenic, and went on board ship with them; *I do not recollect the day. We went on board a three-masted ship. I do not recollect to what nation it belonged. We remained on deck, and did not go below. We remained on board about one hour. Fenic, the father, took us on board; he rowed the boat himself; he ate and drank on board, and then brought a bundle of clothes on shore.*'

"Until this time, neither he nor Villalunga said anything about a bundle of clothes.

"This boy's second evidence thus proceeds:—'I did not understand the language of the people on board the ship; they appeared to speak like Jews or Moors. I did not go on board more than once. When we landed on the wharf, the Maltese,' i. e. Fenic '*gave me some money, a pistoreen, and told me not to say anything to anybody about our having been on board.*'

"The effect which this was designed to produce is obvious, viz. that the ship visited was in quarantine, and Fenic, the Maltese, was conscious that he had committed an offence against the quarantine laws which rendered it necessary for his own safety that he should bribe this boy to secrecy. This story is full of incongruities; it is not probable that a man should select for his Sunday excursion, to eat, drink, and make merry, a ship in quarantine; it is more improbable still that Fenic should

gratuitously place himself in extreme peril, by taking with him (to be witnesses of his offence) children of the artless ages of 10, 11, and 13, on an expedition which in his own judgment, as demonstrated by his own act, he is convinced exposes him to severe punishment.

“But with regard to the ship ‘Dygden,’ I find that she had already received pratique, and had been admitted to free intercourse with the shore, on the 6th of August, *four days previously to the alleged visit of Fenic*, the date of which, notwithstanding Caffiero’s loss of memory on his second examination, had already been ascertained by Villalunga to have been Sunday, August 10th, on which day Fenic, therefore, could commit no crime by going on board; and the story of the bribe and injunction to secrecy resolves itself into a clumsy and ill-disguised attempt at giving a colour of guilt to a fabulous occurrence which, even if it had been real, would have been guiltless.

“His second evidence concludes thus:—‘My mother was a washerwoman, and washed for a black woman who lived next her. Fenic’s wife refused to wash the bundle of clothes that he brought ashore; he offered them to my mother, who also refused them; he then gave them to an Englishwoman: I knew her: *she is dead: I do not know her name, nor where she lived.*’ I find by my notes that he added, ‘This occurred during last winter,’ although the words are not entered upon the minutes. He was then asked, ‘What season of the year was it that you were on board of ship?’ To which he cautiously replied, ‘It was either summer or winter, I believe.’

“Evidence such as this, and given as I saw it given, bears on its face every character of falsehood; and disbelieving as I do this boy’s whole story, and, at the same time, considering his extreme youth, the testimony given by him has upon my mind the further operation of tainting with more than suspicion all the other evidence proceeding from the same class of witnesses, which consisted chiefly of hearsay in conversations with persons who had since died; because it would seem that this child must have been an instrument in the hands of some one of maturer age.”

The suspicion attached to the second appearance of this child is confirmed by a similar re-appearance of Villalunga, who, after sixteen days’ absence from the Board, on the 24th of April, again presents herself as a witness. She now remembers that Mrs. Fenic had asked her to wash some clothes; that she did not wash them, being herself indisposed; but that she was told by Mrs. Fenic that she put these clothes out to be washed.

Mr. Howell thus comments on this second appearance of Villalunga:—

"I have observed that Caffiero added to his original testimony so much as to give to it a new character altogether; I now observe, that six days after Caffiero's amended testimony, and sixteen days after her own original examination, the woman Villalunga comes back with a new story, of which, singularly enough, the principal point is made to coincide with the alterations and emendations in the evidence of Caffiero."

On an examination of the surviving member of the Fenic family, the widow of Fenic himself, it appears that she gave a positive denial to this alleged visit of her husband and children to the ship.

"She was at my desire," says Mr. Howell, "particularly reminded that the duty which she owed to society required her to disclose everything that she knew; and from the ingenuous manner in which her evidence was given, I am led to believe that she spoke the truth.

"She declared that she did not know the cause of her children's illness:—'They were attended by Dr. Lopez, who is dead, and who said they had a tabardillo and indigestion, *caused by eating green figs*. He did not say what was the cause of the tabardillo. My husband was a cigar-maker; but he did not go on board ship either to buy tobacco or to sell cigars. Neither my husband nor my children went into the bay at any time during last summer or autumn. I know this: because, if they had gone, they would have told me, and they did not tell me.' Nor, indeed, is it to be supposed that the children would not have told their mother, and that the husband would not have told his wife, that which all of them are declared to have communicated so freely to other people."

On being cited before a Public Notary at Gibraltar (November 14th, 1829), this witness still more particularly deposed—

"That it was utterly untrue that her husband went on board any ship in the bay at any time last summer; that, on account of his age and infirmity, he had not been in a boat for ten years past; that she is equally certain that her two children never went on board any boat or ship; that, with respect to the boy Caffiero, neither she nor any of her family knew anything about him; and that his story of having gone on board the ship with her husband and her two children, 'is a made-up falsehood.'"

Mr. Howell sums up the result of his examination of the evidence adduced before the Board respecting the Fenic family in the following words:—

"Having thus examined in detail the evidence adduced to connect the illness of Salvador Fenic (the alleged first case of the

epidemic) with the 'Dygden,'—and no other vessel has been pointed at,—I find not only that it completely fails to make out even a *primâ facie* case, but also, from the whole complexion of the evidence, I am convinced that the story of Fenic's visit to that vessel on the 10th of August is, from beginning to end, a fabrication."

Apparently in anticipation of a failure to connect the illness in Fenic's family with a foreign source, much testimony was given before the Board derived, as is stated by Mr. Howell, "through channels most impure," about instances in which foul clothes are supposed to have been brought ashore by sailors arriving from the Havannah, in the early part of the epidemic, and which foul clothes infected the washerwomen.

After showing at some length the discrepancies and contradictions which proved the whole testimony adduced on this point to be utterly worthless, Mr. Howell says:—

"Here I leave the journals of washerwomen, and the tattle of their gossips, remarking this fatal objection to each washing-tub anecdote, however circumstantial, that *not one of them goes back so far as to precede*, and therefore to account for, *the alleged first case of the epidemic*, namely, that of Salvador Fenic, who, as we are told, fell ill *on the 11th of August*, and upon whose single case, therefore, the proof of importation rests. And if the attempt to connect the illness of Salvador Fenic with a foreign source be, as I hold it to be, a complete failure, how is the illness of the boy Caffiero to be accounted for? And to what is to be ascribed the illness of Mr. Martin's child on August 16th, a case quite as early as that of Caffiero, and which has not been attempted to be traced to importation? not one of the washing-tub cases being anterior either to that of Mr. Martin's child or to that of Caffiero, both of which are unquestioned cases of the epidemic."

It was essential to the proof of the connection of the "Dygden" with the outbreak of the epidemic, to establish the fact of the existence of Yellow Fever on board the ship. No proof of this appears to have been adduced.\* On the contrary the captain of the ship declares that no such disease existed on board; the head of the Quarantine department, after an official examination into the fact, affirms that there is no evidence whatever to disprove the truth of the captain's statement, and the Quarantine Medical Officer, after "a minute inspection of the captain

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\* See Appendix I., p. 214.

and crew," states that he "found them all in perfect health."

"I have minutely inspected the captain and crew," he says, "whom I found in perfect health. The reason for putting this ship in Quarantine for 40 days was, that two men died on the passage. It is now 66 clear days since the first man died, and 61 since the death of the last, and nothing like disease has since appeared, nor have I the most distant reason to apprehend danger to the public health from any circumstances connected with the 'Dygden.'"

Mr. Howell calls special attention to this report of the medical officer :—

"This report," he says, "was written, as it strikes me, under circumstances which entitle it to much consideration. This ship had been officially pointed out to him (as the Medical Officer of Quarantine) as being strongly suspected. The responsibility of his office was thus brought fully before his eyes, and he had *then* no motive for making a false report of his inspection of the 'Dygden's' master and crew, because the epidemic had not at that period commenced. If he had observed any reasonable grounds for suspicion, he had only to fall in with the rumour and recommend that none of the persons or susceptible articles on board should be permitted to land. The conduct and declarations, therefore, of Dr. Hennen, as a responsible public officer, under such circumstances, when, if he erred at all, it would probably be on the side of *over caution*, I hold to be most material."

Such is a fair specimen of the evidence adduced on this occasion to establish a positive case of importation. It breaks down at every point. There is complete failure in the proof that Yellow Fever existed on board the ship; there is complete failure in the proof that there was the slightest connection between the ship and any persons on shore, and there is even failure in the proof that the individuals who are alleged to have introduced the disease were really affected with a malady of the same nature as the epidemic that subsequently prevailed.

The Judge Advocate thus states the conclusion at which he arrived after a careful examination of the proceedings of the Commission.

"I am of opinion that the evidence brought forward has totally failed to prove that the late epidemic disease was introduced from any foreign source, either by the Swedish ship 'Dygden,' or by any other means; and I am further of opinion that the late epidemic had its origin in Gibraltar."

Medical observers on the spot, not members of the Board, but who carefully watched its proceedings, it is believed, without any exception, arrived at the same conclusion. Thus Dr. Smith sums up the result of his examination of the subject in the following words :—

“That it was not imported I think every candid man will admit who has deliberately weighed the evidence given on the subject before the Board of Commissioners, and the facts I have stated. Every endeavour to establish the importation doctrine has failed, and both the Colonial Secretary, Sir George Murray, and Sir James McGrigor, Director-General of the Army Medical Department, I have heard, are convinced there is not the slightest ground for such a belief; but, on the contrary, that there is every reason to suppose the disease owed its origin to causes within the walls of the garrison.”

Several comments were made by those who paid attention to the subject at the time, on the manner in which this investigation was conducted, which appear to deserve notice.

Complaints were made that the result of the inquiry was prejudged. In proof of this it was found that the President of the Board a few days before it held its first meeting, addressed to the military secretary of the garrison an official letter in which, among other observations directly tending to a prejudgment of the case, he affirms, that, “the fever in question has often been traced to importation, and against this source *only* must we look for its “prevention.”

It appears further that before the meeting of the Board an official intimation of the views and wishes of the local authorities was promulgated in the Government Gazette, into which nothing is admitted but by authority, in the following words :—

“The scourge from which we have been by Divine Providence just delivered must be an exotic of some kind. It is in its origin independent of everything inherent in the soil which we inhabit, incapable of existing among us during the winter months, and totally distinct from and unconnected with the Remitting and Intermitting Fever, which may be said to be unknown in this garrison.”\*

“Two causes,” observes Mr. Howell, “concurred to operate

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\* See Note, pp. 94, 95.

injuriously upon the proceedings of the Board: *First*, the conviction universally prevalent among the *civil* population of Gibraltar, that the prosperity of that community would be undermined if it should be proved that the epidemic had been generated on the spot, because of the prohibitions and restrictions which it was anticipated would in that case be inflicted upon its commercial intercourse with other places. Hence the notion that not only the last epidemic, but that all its predecessors had been imported from some foreign country was not only anxiously supported by the unanimous voice of the civil community, but it was with equal unanimity believed that a different doctrine would be fatal to the commercial prosperity of the place. From this feeling of self-interest it is to be admitted that the *military* were exempt, a distinction between the two classes which ought to be taken into account in estimating the value of the evidence taken by the Board, and more especially the evidence of the medical practitioners.

"The *second* cause operating injuriously upon this inquiry, was the publication, in the official Government newspaper, (into which nothing is admitted except by official authority), on January 12, 1829, of an article authoritatively announcing that the late epidemic had been imported into Gibraltar, and denouncing as void of common sense any person who should hold a different opinion. This official notification of the feelings of the local Government (preceding as it did by only 12 days the appointment of the Board of Inquiry) could hardly fail to encourage evidence on one side, and discourage evidence on the other."

Complaints were also made that there was a partial selection of witnesses.

"It always appeared most extraordinary and unjustifiable," says Dr. Gillkrest, "that on this kind of inquiry, which was intended by the Secretary of State to be so beneficial to the interests of humanity, the Superintendent of Quarantine as president, should have assumed the right in several instances of selecting the witnesses, which obviously prejudiced the question, and by which much of the truth was intercepted.

"Several medical officers of the garrison who had much experience respecting the progress of the epidemic, were either not examined at all, or only in a very imperfect manner. I was among the latter, being surgeon to the 43rd Regiment, and present during the whole epidemic. After a very limited examination, I officially informed the President, by letter, that I had much to state; but like others, I was not called afterwards.

"From what I felt due to the service of which I had been a member for so many years, as well as the cause of truth, I was induced to protest against such proceedings, which protest will, I presume, be found with the documents connected with the inquiry forwarded from Gibraltar to the Colonial Office in London."

Complaints were further made of the mode of collecting the evidence adopted on this occasion, which was such as to excite the suspicion of some of the members of the Commission, and to lead eventually to their condemnation of it, and their repudiation of the Report which was founded upon it. *See* Letter of Sir George Murray, and reply of Colonel Chapman, the Civil Secretary, p. 274 ;—also Report of Judge Howell, Appendix, II. pp. 245, 273.

We need pursue no further this examination of the cases of alleged importation. We apprehend it will be apparent, from the specimens we have adduced of the kind of statements on which they rest, that no one would trust to such evidence in the ordinary concerns of life ; and yet the belief in the fact of importation, and the proceedings actually consequent on that belief, involve questions of health, property, and life, to an incalculable extent ; and, we submit, that the clearness and weight of evidence required to justify such proceedings, should be proportionate to the momentous consequences that may result from them.

When a disease suddenly breaks out in a town or district, and such disease is believed to be of foreign origin, and highly contagious—

“A cordon,” says Dr. Smith, “is immediately established, the inhabitants are shut up, all communication cut off, and those that are not fortunate enough to escape before alarm is excited in all probability fall victims to the disease. Fortunately for us in Gibraltar, we have had the means of escape in our hands, and a removal to the Neutral Ground, Europa Flats, Windmill Hill, or the Bay, ensures safety. But this is not the case in Cadiz and many other large towns. There the inhabitants must patiently wait their fate, like people in a house on fire with the doors and windows shut to prevent their getting out..”

“An opinion,” says Mr. Amiel, “has been very prevalent that the epidemics which have appeared in this garrison and on the coast of the Peninsula have constantly been imported in vessels from Vera Cruz, Havannah, or the West India Islands. But this opinion appears to be grounded rather on popular prejudices and the illusions of terror than on philosophical investigations. \* \* \* \* The arrival in this garrison of one or more persons ill of this malignant fever at the breaking out of the epidemic, will not prove its foreign origin nor its propagation by contagion ; for, on the one hand, fever has broken out where

there was not the least possibility of foreign introduction, and on the other, a number of people labouring under it have sometimes been landed in other places without injury to the health of the inhabitants."

"The only instance of public precautionary measures," says Dr. Blair, "being adopted under the impression that the epidemic disease was contagious, was at Berbice, at Fort Cange. Captain Warburton, at the instance of assistant surgeon Turner, established a rigid *cordon*, and prevented all intercourse with the town of New Amsterdam. Previous to this no case had appeared in the garrison. An effect of the cordon was to prevent huxters and others bringing in fresh provisions, fruit, &c., to the soldiers. The soldiers were also of necessity thrown on their own resources *pour passer le temps*."

"However, notwithstanding all communication having been cut off with the town, the Yellow Fever epidemic soon appeared in the garrison, and poor Dr. Turner fell a victim—not to the Yellow Fever, according to the practitioner who attended him, but—to inflammatory fever, the result of extreme fatigue, mental anxiety, and chagrin."

"The infractions, whether direct or open," say the Committee of Physicians of Barcelona, "of the strict cordon by which the place was surrounded, gave occasion to the people themselves to turn it into ridicule, bestowing upon it the most contemptuous epithets."

"The vexations experienced by those who quitted Barcelona, and the arbitrary measures adopted by each separate community, even in the highest mountains, against an imaginary contagion, were an insult to humanity, and a proof the most authentic of the ignorance in which nations may be plunged by the vicious routine of sanitary [quarantine] laws."

"In every clime," observes Humboldt, "men fancy to derive consolation in the idea, that a disease which is considered pestilential has been brought from abroad. This belief flatters the national pride. To inhabit a country which produces epidemics might be deemed a humiliating circumstance; and it is more satisfactory to consider that the malady is a foreign one, and that its breaking out has been merely the effect of an accident against which it will be easy to guard in another instance. The people immediately adopt this explanation of the origin of the disease, because it is easily comprehended. The medical men, on their side in general, rest satisfied with it, because the word importation relieves them from all responsibility, and from the trouble of investigating the nature and real cause of the disorder. From this has arisen that remarkable facility with which the doctrine of importation has been eagerly received by all classes when an epidemic manifests itself in a country; and a vessel, a traveller, or a parcel of goods arrive at the same time. So it is that the Havannah, Vera Cruz, and the sea-port towns of

the United States, constantly accuse each other of the importation of the Yellow Fever during the summer months, just as the inhabitants of Egypt refer to the arrival of Greek vessels the appearance of the plague; when in Greece and Constantinople, the disease is attributed to vessels coming from Alexandria and Rosetta."

With reference to several of the preceding cases of alleged importation, the apparent want of skill on the part of the medical inquirer in examining witnesses on questions of fact, and extorting the whole of the truth from the reluctant, the intimidated, the prejudiced, or the interested, can scarcely have escaped attention; but there would seem to be a peculiar disadvantage for the discovery and statement of the truth, on this subject, under which the medical departments of the army and navy are placed—that the discipline which, in other respects, is so essential to the service, naturally and often, perhaps unconsciously, predisposes the subordinate officer to adopt the views, and support the opinions which are understood to be most acceptable to the head of the department upon whom his future professional prospects mainly depend. To the honour of both services, it must be stated that examples are of constant occurrence in which all considerations of this kind are disregarded; but such observations as the following could scarcely have been made, if the difficulty were not a real, and sometimes a very oppressive, one.

"At one period of medical rule in this garrison," says Dr. Smith, "every variety of fever was ordered to be returned under one head. Such indeed was the thralldom of the military medical press (if I may so use the expression) in Gibraltar, from the termination of the epidemic fever of 1814, until the arrival of the late Dr. Hennen, in 1826, that it was considered a most wicked heresy for the surgeon of a corps to return fever under any other head than simple-continued fever. The consequence is, that although febrile diseases are the most frequent of the numerous diseases treated in the Military and Civil Hospitals, no correct table of fevers can now be framed from the returns of these establishments, no distinction whatever being drawn between the different species of remittent and continued. The authors of such a measure can best answer for themselves. To me it appears to have been an attempt, and one too successfully made, to obstruct the advance of truth, short of nothing but the direct application of the bayonet's point; and it is due to the memory of Dr. Hennen to state that

he had not assumed the charge of the department two weeks before he abrogated this mischievous state of things. This measure soon showed that neither the intermittent, remittent, or continued Yellow Fever, were such uncommon forms of disease in the garrison, as Sir William Pym and others of his school, had, for a series of years, endeavoured to represent. Instead of remittents being rare, and intermittents never occurring, in Gibraltar, Dr. Hennen says, at page 119 of his *Medical Topography*, 'I have no hesitation in asserting, that remittent fevers are of frequent occurrence\* (speaking of Gibraltar), that the bilious autumnal remittent is annually met with, and that cases of genuine Yellow Fever (as described by writers on diseases of the West Indies), accompanied by true black vomit, occur both in the Civil Hospital and in private habitations.'

And a little further on, in the same paragraph, he adds:—

"It has been asserted, that these fevers are solely the produce of the Neutral Ground and the Lighters in the bay; but annual proofs subversive of this hypothesis are not only familiar, but on record.' I can with confidence," continues Dr. Smith, "add my own testimony to the truth of these observations. I have already this year (1830) seen three well-marked cases of sporadic Yellow Fever in the garrison, two of them with black vomit."

Mr. P. Wilson, then on the half-pay of the Army Medical Staff, and Surgeon to the Civil Hospital,† wrote to Dr. Chervin, on declining any further part in the investigations made in 1828-29, as follows:—

"I saw daily, more and more, the disposition and actual attempts to torture facts to the contagious side of the question; and, therefore, as I could not submit to the idea of putting my name to papers officially drawn up in this partial way, I thought it best to withdraw myself entirely.

"In candid inquiry I will go as far as any man, ability permitting, but to be led by one name, or another name, or for opinion, or for the prospect of gaining admission at preferment's gates,—no, Sir! to such I shall never prostitute my independence."

Persons who see disease only on a small scale, and who observe that within that small circle one attack is often followed by another in the same family, and that by a third, and so on, naturally believe that the second case is caused by the first, and the third by the second, and thus

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\* See Note, pp. 94, 95.

† Now Surgeon to the Colonial Hospital, New Zealand.

refer the whole series of events to contagion. It is on this description of evidence that the entire structure of Quarantine is based. It is only when the facts as to the spread of disease on a large scale are examined ; when it is found that the same disease breaks out at the same time in different and distant towns, and in different and distant parts of the same town, and that wherever the disease rages, there invariably the localizing conditions are similar, that the first wrong impression is corrected, and a juster conclusion formed. The fault of persons in authority is, that they do not investigate the operation of causes in a wide field, and that they trust to the statements of quarantine officers, who are necessarily confined to a narrow view, and who are, moreover, biassed by their own peculiar interests against extending their sphere of observation. The result is, that investigations on the question of importation and contagion have seldom been of such a searching character as to reveal the whole truth, and that several most valuable opportunities of settling the question have been lost.

We submit as the general result of the preceding evidence, illustrative of the character, mode of propagation, and localizing conditions of Yellow Fever, the obvious conclusion that it is even less reasonable to expect that quarantine or sanitary cordons can have any real influence in arresting its progress, than it is to place confidence in such expedients in Epidemic Cholera. The following testimony borne by Dr. Davy to the uniform and signal failure of such measures in Yellow Fever, is in accordance with that expressed by every observer, with scarcely a single exception, who has been brought into extensive and practical acquaintance with the disease.

“ Of the Inspectors General of Hospitals who have served in the West Indies during the last 40 years, amongst whom the respected names of Sir Charles Ker, Dr. Jackson, and Dr. Ferguson, are prominent, all have been persuaded that Yellow Fever, including many varieties or modifications, is of local origin, and is not propagated by contagion, with the exception of one, Mr. Green, who was a strict contagionist, and who, during the epidemic of 1819, a fever of wider range than is usual, acted accordingly, attempting even the enforcing of quarantine regulations, after the manner practised in the Mediterranean, where

he had previously served and witnessed Plague. The vexatious and futile character of these measures are not yet forgotten in Barbados. Before entering on an undertaking of this kind, even supposing the disease to be guarded against to be contagious, it would be well to consider the extent to which it is practicable. If the doctrine of contagion be false, how many are the evils unavoidable from the attempt to enforce quarantine: these are strongly exemplified in every account we have from eye witnesses of the Plague; comprised in panic, desertion of the living, neglect of the dead; in brief, horrors on horrors, and even crimes on crimes."

In our Third Report we propose to consider the decisions of the International Sanitary Conference which has just terminated its labours at Paris. In the mean time we may be permitted to observe that the extent of reform in quarantine against Yellow Fever proposed by the Conference, sufficiently demonstrates their opinion as to the excessive and needless severity of the present practices. This may be judged of by the fact, that before the Conference commenced their labours, all arrivals from countries subject to Yellow Fever were put in quarantine in different ports of the Mediterranean, extending from 3 to 21 days for passengers and ships, and varying from free pratique to 28 days quarantine on goods; while it has been proposed to substitute for these irregular and excessive interruptions to commerce, a constant quarantine of from 3 to 7 days for all arrivals on board of which no cases have occurred during the passage, and from 7 to 15 days when cases have occurred. They have proposed, moreover, that quarantine should cease when the disease has ceased.

Even this mitigated quarantine, however, has been based chiefly on general assertions as to its utility, unsustained by any evidence; the only cases which were brought prominently forward in proof of the importability of Yellow Fever, and the consequent necessity for quarantine regulations, having been those cases which we have by anticipation examined and refuted in this Report.

From a consideration of the whole or the preceding evidence respecting Yellow Fever, we have arrived at the following conclusions:—

1. That Yellow Fever Epidemics break out simultaneously in different and distant towns, and in different

and distant parts of the same town, often under circumstances in which communication with infected persons is impossible.

2. That Yellow Fever Epidemics are usually preceded by the occurrence of individual or sporadic cases of the disease, which sporadic cases are likewise common in seasons when no epidemic prevails.

3. That Yellow Fever Epidemics, though occasionally extending over large tracts of country, are more frequently limited as to the space over which they spread, often not involving the whole of a town, and sometimes not even any considerable district of it.

4. That Yellow Fever Epidemics do not spread from district to district by any rule of gradual progression, but often ravage certain localities, while they spare entirely, or visit very lightly, others in the immediate neighbourhood, with which the inhabitants are in constant intercommunication.

5. That Yellow Fever Epidemics, when they invade a district, do not spread from the houses first infected to the next, and thence to the adjoining, and thus extend as from a centre; but, on the contrary, are often strictly confined to particular houses in a street, to particular houses on one side of a street, to particular rooms in the same house, and often even to particular rooms on the same story.

6. That in general, when Yellow Fever breaks out in a family, only one or two individuals are attacked; commonly the attendants on the sick escape; and when several members of a family are successively attacked, or the attendants on the sick suffer, either the epidemic was general in the locality, or the individuals attacked had gone into an infected district.

7. That when Yellow Fever is prevalent in a locality, the most rigid seclusion in that locality affords no protection from the disease.

8. That, on the other hand, so great is the success attending the removal from an infected locality, and the dispersion of the sick in a healthy district, that by this measure alone the further progress of an epidemic is often arrested at once.

9. That such dispersion of the sick is followed by no

transmission of the disease, not even when the sick are placed in the wards of a hospital among patients labouring under other maladies.

10. That no one of the preceding facts can be reconciled with any other conclusion than that, whatever may be the exciting cause of Yellow Fever, it is local or endemic in its origin: and the evidence of this conclusion is therefore cumulative.

11. That the conditions which influence the localization of Yellow Fever are known, definite, and, to a great extent, removable; and are substantially the same as the localizing causes of Cholera and of all other epidemic diseases.

12. That, as in the case of all other epidemic diseases, in proportion as these localizing causes are removed or diminished, Yellow Fever ceases to appear, or recurs at more distant intervals, and in milder forms.

13. That besides the common external localizing causes, there is one constitutional predisposing cause of paramount importance, namely, non-acclimatization—that is, the state of the system produced by residence in a cold climate; in other words European blood exposed to the action of tropical heat; the practical lesson being that the utmost care should be taken to prevent individuals or bodies of men, recently arrived within the Yellow Fever zone, from going into a district in which the disease actually exists or has recently been present.

14. That there is no evidence to prove that Yellow Fever has ever been imported.

15. That consequently the means of protection from Yellow Fever are not quarantine restrictions and sanitary cordons, but sanitary works and operations, having for their object the removal and prevention of the several localizing conditions, and when such permanent works are impracticable, the temporary removal, as far as may be possible, of the population from the infected localities.

We deem it our duty to state, in conclusion, that from the most careful examination which we have been able to make of the mass of evidence submitted to us from which the foregoing conclusions have been deduced, we have not found a single fact or observation clearly ascer-

tained and authentically recorded opposed to the general tenor of such evidence. We have met with no exceptional cases. We have indeed found the opinions of some authorities, for whom we entertain great respect, not in accordance on some points, but these have reference for the most part to matters of a purely professional and scientific nature. On the great practical question, whether, whatever may be the nature and mode of propagation of Yellow Fever, Quarantine and Sanitary Cordons can afford any real protection against its introduction and spread, we believe there is now a very general unanimity of opinion, in accordance with the evidence we have submitted, that they cannot. We believe there is the like general agreement in this further practical conclusion that, the substitution of Sanitary or Hygienic measures, for Quarantine isolation and restriction, would afford more certain and effectual protection.

We have received from recent inquiries much information with reference to the Plague of the Levant, the results of which we propose to state in our Third Report; results which appear to us to be of great practical value, as showing on the one hand what measures experience has proved to be inefficacious, and even mischievous; and on the other hand, what measures may be resorted to with the best hope of preventing the outbreak, or arresting the progress of this formidable disease; and thus of superseding the necessity of those grievous interruptions to commerce, and international communication, which quarantine, so universally imposed on account of Plague, has hitherto occasioned.

All which we humbly certify to Your Majesty.

SHAFTESBURY.

EDWIN CHADWICK.

T. SOUTHWOOD SMITH.

*Whitehall, 7 April 1852.*

## APPENDIX No. I.

## REPORT ON YELLOW FEVER.

BY DR. GILLKREST.

## INTRODUCTION.

*London, 28th Oct., 1850.*

MY LORDS AND GENTLEMEN,

THE wisdom of instituting a spirit of close investigation in all matters connected with those diseases which, in an epidemic form, sweep away human beings in masses, has been singularly exemplified throughout the country during the late prevalence of cholera; for though great the afflictions lately experienced, the wisdom, energy, and humanity so promptly put forth were certainly the means of always mitigating, and, in numberless instances, of limiting this scourge, which otherwise would have produced results calamitous beyond calculation. Let us hope that the power which worked so well in the cause of truth on that occasion, may not rest in the belief that all has yet been done which the interests of humanity require.

Contagion in Yellow Fever still remains to England a question of great national importance, on account of her colonies,—her commerce,—her fleets,—and her armies.

Volumes have been written in various languages in support of the fallacy which annually consigns numbers of our fellow-beings to inevitable death.

It will be seen from what I take the liberty of submitting in the following Report, how much humanity needs protection from opinions so erroneous and destructive.

Great have been the efforts of many eminent men to disabuse the public mind on a question of such vital interest. But pre-eminent I may assert were the labours of the late Dr. Chervin, of Paris, on this subject;—respecting whom I beg to give the following extract from the Report of a Commission appointed by the Academy of Sciences in Paris, in 1827, to adjudge the Montyon prize designed for labours in medical science. The Commissioners were MM. Portal, Boyer, Chaptal, Dumeril, Dulong, Gay-Lussac, de Blainville, Frederic Cuvier, and Magendie.

The Report made by the above gentlemen, after stating some unusual steps taken by Dr. Chervin, to ascertain the contagious or non-contagious nature of the Yellow Fever at Gua-

daloupe, to which place he had proceeded from Paris, for the sole purpose of making investigations,—proceeds thus:—"This is nothing!—It was, on the contrary, then, that Dr. Chervin conceived the wisest and vastest plan, that ever a medical man formed for the interests of humanity.

"It was no longer sufficient for him, that he had satisfied himself that the Yellow Fever was not contagious in Guadeloupe; it became necessary to ascertain whether it did not possess that character in other localities and in other latitudes and climates. It was, above all things, necessary to convince the Governments of Europe, so that commerce might be freed from unnecessary precautions, felt to be burthensome, and that nations might be saved great expense in sanitary establishments. In attaining his object, Dr. Chervin was only impelled by his ardent philanthropy—no other means, but the sacrifice of his patrimony—no support, but his own inclination and physical powers—let it be declared, to the honour of humanity, that by such means alone, enterprises of this kind could be accomplished; and in fact, what a powerful Government could scarcely hope to obtain at great expense, Dr. Chervin proposed to himself to obtain.

"Dr. Chervin performed this gigantic undertaking, to which the history of medicine furnishes no parallel, in a fortunate manner, but with unheard of efforts, and perseverance above all praise."

Speaking of his having collected the evidence of hundreds of medical men in all parts of the world, where the Yellow Fever is known to prevail, the Report continues—

"He visited, in eight years, all the Colonies belonging to France, England, Spain, Holland, Denmark, Sweden;—he visited all parts of North America, where the Yellow Fever had shown itself, from New Orleans to Portland, in the state of Maine;—so that from Cayenne to this last place, he traversed over and made investigations in 37 degrees of latitude."

"It [the Commission] therefore proposes to adjudge him a prize of 10,000 francs; undoubtedly a poor reward for the many sacrifices which he must have made; but when a person has, like Dr. Chervin, merited so much from science and humanity, and shown such disinterestedness, *on voit la Couronne et non pas sa valeur.*"

Strongly as I believe that many have been unreasonably biassed in their opinions as to the contagious nature of Yellow Fever, yet I most readily admit that others, among whom are some of my personal friends, have been misled by the *semblance* of contagion, so frequently occurring, and by their insufficient investigation of all the circumstances connected with the subject.

Indeed, I think from what will be found in the following

pages, that this disease would seem to require a very special study as to its nature and the phenomena it presents.

Those who have laboured with me in my investigations can, I trust, speak as to the good faith with which those labours have been conducted.

In conclusion, allow me to entertain the hope that what I have here submitted, under the sanction of your honourable Board, as a bequest to my country, will tend, in some degree, to excite the attention of Her Majesty's Government, and of the Legislature, to a more efficient investigation of the subject;—and that the same humane as well as energetic steps which, in so timely a manner, crushed the head of the hydra “Contagion” in Cholera,—will no longer suffer a system to have sway which continually places at the mercy of persons prejudiced, or deeply interested, or little acquainted with the subject, the mighty question of the lives of large portions of our naval and military forces in various parts of the world.

I have the honour to be,

My Lords and Gentlemen,

Your obedient Servant,

J. GILLKREST, M.D.

Inspector-General of Army Hospitals, and Corresponding  
Member of the Paris National Academy of Medicine.

*To the General Board of Health.*

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## REPORT.

I HAVE taken upon me the very onerous task of writing on Yellow Fever, which, in its malignant form, has swept off numberless human beings in the West Indies, North and South America, various parts of the coast of Africa and adjacent islands, as well as Gibraltar, and several places in Spain; once in Lisbon (1723), and once in Leghorn (1804.)

I cannot better convey an idea of the difficulty of my undertaking than, by quoting from the French Academician MOREAU DE JONNÉS,\* 1820:—“Several powerful causes have contributed to render Yellow Fever a disease of which the origin is uncertain, the cause unknown, the character equivocal, and the consequences so terrible, that we are equally at a loss to discover the means of preventing it, of checking it, or of treating it.”

With certain modifications regarding the checking of the disease, and perhaps also as to its treatment, unless, indeed, in the form the most malignant of all—I fully agree in the pro-

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\* A man of science, formerly a military man, who, since his service at Martinique in 1802, as aide-de-camp, has figured a good deal in the discussions relative to the contagion of yellow fever, as well as spasmodic cholera, though not of the medical profession.

priety of what is here said, being conformable to my great experience, aided by laborious researches in the course of a long life.—I cannot admit that it has not often been prevented or checked by change of station.

### NAMES GIVEN TO THIS DISEASE.

The Academician already mentioned, has enumerated no fewer than 36 names for the disease in different languages, of which it may be sufficient to mention the following: "Yellow fever," "Black Vomit fever," "Bulam fever," "Fièvre jaune," "Vomito negro," &c. Some have adhered to Bilious Remittent, others have called it Pestilential, from its ravages, and a late writer denominates it the "Hæmagastric Pestilence," from a single symptom which, however, is far from constant.

The anomalies which this disease has been observed to present,—the absence, under the observation of one medical man, of some of the symptoms which, during another epidemic, had been well marked,—the fact of practitioners having observed that certain symptoms, prominent during one period of an epidemic, have at another period, been totally absent; the fact, too, of patients in the same ward of an hospital, being found to labour under symptoms so variously grouped as to lead an inexperienced practitioner to believe that he had before him three or four diseases bearing little affinity to each other;—all these circumstances have thrown difficulties in the way of this disease having had a place assigned to it in nosological arrangements, free from objections.

By some, Yellow Fever has been classed with *continued* fevers, the symptoms not having appeared to them to correspond with those laid down by nosologists as characterizing remittents; while, according to the statements of others, of whose accuracy of observation there cannot be a doubt, the disease has assumed the most unequivocal remittent form: indeed, there is very respectable evidence in proof of its having, on some rare occasions, assumed even the character of an *intermittent*.

This is not the place to define what constitutes, rigorously, remittent fever; it is plain that in most countries the opinions of medical men are at variance upon the subject. The remarks of close observers go to prove how frequently remittents may be *masked* so as to mislead as to their true character.

The great Italian medical authority Tomassini says on the subject of classing diseases:—

"Tant il est vrai que l'entreprise de classer les maladies a souvent entraîné dans des contradictions les auteurs les plus éclairés."\*

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\* Tomassini's Researches on Yellow Fever, p. 24.

Let us listen to what the experience of Dr. Elliotson, of St. Thomas's Hospital, had taught him on the subject of remittents:—

“Remittent fever will frequently occur insidiously, and, unless you are quite up to it, you may as easily pass it over as some forms of epilepsy. I have had many cases of remittent fever, which, in addition to the symptoms of continued fever, were merely characterized by excessive sweating.”—*London Medical Gazette*, January 3, 1831.

Those therefore cannot, with justice, be accused of much inaccuracy who, confining themselves probably to their own field of observation, have looked upon Yellow Fever as belonging to the class of continued fevers; but it is important to show whether those are right who maintain that the disease bears no affinity whatever to remittents, and that it *never* assumes any other than the continued form.

On this point it will be right to quote authorities of respectability. Dr. Rush, in his account of the epidemics at Philadelphia in 1793 and 1794, distinctly notices remissions in several pages:—“The remissions were more evident in this than in the common bilious fever. They generally occurred in the afternoon.”\* “It,” speaking of delirium, “alternated in some cases with the exacerbations and remissions of the fever.”† Speaking of the second form of this fever, he says that it was attended “with obvious remissions.”‡ At p. 45 of his account of the epidemic of 1794, he says that the disease “appeared most frequently in the form of a remittent. The exacerbations occurred most commonly in the evening.” In another passage, often quoted, “Never has the unity of our autumnal fever been more clearly demonstrated than in our present epidemic. Its principal grades, viz., the intermittent, the mild remittent, the inflammatory bilious fever, and the malignant Yellow Fever, have all run into each other in many instances. A tertian has ended in death with black vomiting, and a fever, with the face and eyes suffused with blood, has ended in a quotidian which has yielded to a few doses of bark.”§

Dr. John Hunter in his *Observations on the Diseases of the Army in Jamaica*, 1796, p. 62, says, “The fevers that prevail in Jamaica are either of the intermittent or remittent kind. The remittent fevers are both the most frequent and the most fatal.” At p. 63 he describes the ordinary symptoms; and at p. 318 he says, “When the remittent fever is of the worst kind the yellowness of the skin occurs most frequently; but

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\* Account of Epidemic of 1793, p. 79.

† Op. cit., p. 62.

‡ Ibid., p. 82.

§ Letter to Dr. Miller, *New York Medical Repository*, vol. vi., p. 249.

the Yellow Fever is neither a different disease, nor do all the sick, even in fatal cases, turn yellow; yet, at such times, it has been customary to call the prevailing fever by the name of the *Yellow Fever*."

In some observations on Yellow Fever by Staff Surgeon Nodes Dickenson (*Med. Chir. Jour.* Vol. ii., 1820, p. 190—1,) we have the following:—

"Feelings of heat and slight chilliness alternate for a time."

"The truth seems to be that the morbid actions constituting Yellow Fever are marked by almost inconceivable rapidity, and the duration of the cold stage is proportionably evanescent; but generally not the less real; though, from occurring in the night, it is often unnoticed by the patient, and at so early a period, it is seldom observed by the practitioner."

"In the situation of most of the West India sea ports, the continued form is most frequently observed. But we have often remarked that the same exposure and irregularities exercised in places notoriously abounding with marshy effluvia (as the towns of Point à Pitre, Guadaloupe, Fort Royal, in Martinique) have been succeeded by the recurrent character of attack."

Mr. Campbell, of the army medical department, in an official report dated Montserrat, 1825, gives as his reasons for thinking that the remittents, &c. of the West Indies are grades of the same disease—that, during a yellow-fever epidemic at Barbadoes in 1821, he observed, "the most marked difference in the type and symptoms of cases of patients from the same barrack or hut, where not the slightest doubt could be entertained of the disease being produced in both instances by one and the same morbid cause, yet so modified by physical causes, connected with the patient, as to appear quite different diseases, and certainly requiring different modes of treatment."

The frequent occurrence of Yellow Fever in Spain, entitles the statements of the medical men of that country to great consideration on the point in question. The late Dr. Arejula, who was so familiar with Yellow Fever as it appeared in Spain within the last half century (having been appointed by the Spanish Government to visit and report upon various epidemics prevalent in the different provinces, 1803-4), repeatedly informed the writer of this Report that the disease frequently assumed the most marked remittent form. In his description of the Malaga epidemic of 1803,\* he tells us that the bark was found useful during the *remissions*. At p. 71 he is clear on the subject of remissions; and at p. 139 informs us that the disease "without doubt deserves the name of remittent fever."† He even says, when describing a black-vomit

\* See his work on Yellow Fever, p. 25.

† "Merece sin duda el nombre de calentura remitente."

epidemic, "the termination of our remittent in *intermittent*, which also occurred in some instances at the close of the epidemic, was an indication that the disease was about to be extinguished."

The recorded opinions of Dr. Velasquez of Seville, are fully in corroboration of the statements of Arejula. The following physicians were contemporaries of the latter gentleman, and had witnessed some of the Yellow-Fever epidemics of Spain; Dr. Balmis, who called the disease, as it presented itself during the Cadiz epidemic of 1800, "a putrid malignant remittent;" Dr. Flores Moreno, who describes in his work "accessions and remissions;" Dr. Alfonso de Maria of Cadiz, says, "when the yellow fever degenerated into intermittent." In the third volume of Hurtado's *Decadas*, published at Madrid, may be found a memoir relative to one of the Seville epidemics, with the signatures attached of Drs. Gabriel Rodriguez, Serafin, Adamo, Velasquez, and Chicon, to the effect that "sometimes, though rarely, the fever presented itself following the type of an intermittent."\* In the *Trozos ineditos* of Dr. Salva, professor of medicine at Barcelona, evidence is to be found of the disease having been observed to assume the remittent form.

It may be stated that the records of the Gibraltar Yellow-Fever epidemics furnish the following names in support of the fact that remissions not unfrequently take place in this disease: Drs. M'Mullin and Browne, Messrs. Sproule, Wild, Martindale, Amiel, Dow, Donnett, Humphries, Lee, and Hugh Fraser.

For myself, the ordinary characters of remission did not so appear in 1828 as to warrant my speaking decidedly on the subject. If we keep in mind the quotation I have given from Dr. Elliotson as to the characters of remissions being often insidious, and easily escaping notice; and if, also, it be considered that my regiment (43rd) had been more severely and at an earlier period attacked than any other in the garrison, the pressing wants of the sick might well account for such light and evanescent symptoms passing unobserved.

But however lawful it might have been in the midst of clashing opinions to doubt the existence of Yellow Fever in a remittent shape, late authorities in some of the French West India islands, and in the United States, have placed the matter so entirely in the affirmative, that there is no longer room for cavilling on the subject. The evidence, indeed, is so overwhelming, that, extremely reluctant as I am to heap evidence on what is already proved, I cannot forbear referring to the following, the importance of which will be duly appreciated by the profession.

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\* "Alguna vez, aunque rara, se presenta la calentura siguiendo el tipo de intermitente."

I would especially call attention to two memoirs by Dr. Rufz, Paris, 1842, on the Yellow Fever epidemic of St. Pierre, Martinique, prevalent from 1838 to 1841.

I must confine myself to the statement, page 33, of his having "seen no cases *favourable to contagion*;"—also, page 57, that from September 1839 to the end of December 1840, the epidemic at St. Pierre, in the greater number of cases, assumed an intermittent or remittent form, "*bien prononcée*." The sulphate of quinine employed in such cases, appears to have been efficacious.

He states that the surgeon of the frigate "*Herminie*," treated, in September 1838, at Vera Cruz, above 100 cases of intermittent "*vomito*." I would refer the profession at large to these memoirs, in which will be found superabundant proofs of the relationship between the Yellow Fever and the *periodical forms of fever*.

The Reporters, Drs. Londe and Chervin, refer also to the memoir of Dr. Dutroulau on the subject of a similar epidemic at Fort Royal, Martinique, with well-marked remissions.

M. Bertulus, in his Memoir on the Importation of Yellow Fever into Europe, says (p. 66): "These symptoms" [*i. e. general*] "continued from twenty-four to thirty-six hours, sometimes even longer, and were accompanied by an extraordinary alteration of the countenance; then true paroxysms of pernicious intermittent fever speedily appeared, and their stages, though at times sufficiently distinct, in the great majority of the cases ran into each other; these paroxysms succeeded one another with such rapidity, that it was really impossible to detect the moment of intermission, which was looked for with impatience, in order to administer the sulphate of quinine."

A work published by Dr. Bartlett, Professor of Medicine, of Transylvania College (*Phil<sup>a</sup>*, 1847), on the history, diagnosis, and treatment of the fevers of the United States, is full of information on the subject, and must henceforth be appealed to by all who desire information on the Yellow Fever. We are there told (what indeed was always plain enough) that the best description of *periodical fevers* is not to be learnt in England, where, in fact, the "*pernicious forms*" so fully detailed by Torti, Ramazzini, Baglivi, Lancisi, and other old Italian writers, are not to be found in the experience of practitioners.

Amidst the great variety of information on the subject of American fevers, Dr. Bartlett, refers to the following, from a paper by Dr. Lewis, of Mobile, who attended during the epidemic of 1843 cases in the southern part of the city, where the remittent fever prevailed extensively both among the natives and acclimated. Dr. Lewis describes a form of disease which he calls *remittent* and *intermittent* Yellow Fever,, and tells us that

16 cases of remittent\* or intermittent fever assumed the rank and grade of Yellow Fever, and that these cases were all amongst the unacclimated. He estimates the number of cases during that epidemic (1843), at 100, 50 of which proved fatal.

Among the variety of forms shown by Dr. Bartlett to be assumed by the periodical fevers of America, we find (p. 354) the "*algid* variety," which was observed by myself and others in the epidemic at Gibraltar in 1828, and many years ago touched upon by me in writings published in England† and France, in the following terms:—"The patient, although in possession of his faculties, lies for the most part on his back, in a state of collapse, his limbs pulseless, clammy, and stricken with a degree of coldness considerably below that which is found to take place in a corpse under similar atmospheric temperature; while probably he complains of agonizing internal heat, and casts off the bedclothes incessantly." Still stronger, however, is the following quotation from a manuscript drawn up by me in the hospital surgery, on the termination of the epidemic; drawn up to the knowledge of every regimental medical officer present, all being invited to point out any errors in my statements respecting their particular corps: "Singultus," I state, "in a severe degree may now be looked for; the pulse can no longer be felt at the wrists, the extremities become almost as cold as marble in winter, indeed, many degrees colder than any substance which one can lay his hand upon at the time, or than the same parts are when life becomes extinct. This coldness, with a remarkable clammy feel at the same time, conveys a peculiar shock to those who apply the hand to the limbs."

Among other *algid* cases witnessed in the garrison, were two of a very remarkable nature, which appeared in the hospital of the 42nd regiment, then, on account of the illness of assistant surgeon McGregor, under medical charge of Mr. George Brown,‡ assistant surgeon 43rd regiment, who requested me to visit these patients in company with himself.

This gentleman has lately confirmed my strong expressions on these cases. The very mention of them, even at this distance of time, seems to occasion a most painful impression on his feelings. In one of them he speaks of "the shock conveyed by the *intense coldness* of the patient's body,—of the pulselessness,—the broken voice, as in *algid* cholera,—the perfect soundness of mind and readiness to reply rationally to all questions,—whilst, to his utter astonishment, the body was pale as the whitest marble;"—so that, on the whole, an appearance was presented quite "beyond anything" which he had ever before or since wit-

\* See also *Thomas de la Fièvre Jaune*, p. 35.

† *Cyc. Prac. Med.* (1833), Art. *Yellow Fever*; where the general symptoms and morbid appearances of the disease are detailed. It is therefore unnecessary that I should repeat them here.

‡ Now surgeon of the Grenadier Guards.

nessed. Mr. Brown thinks the patient lived two days in this state.

In his second case the symptoms were very similar, with the exception of the extreme paleness, instead of which there existed a tendency to a certain livid or leaden colour,\* especially of and about the ears, this being indeed the most common appearance presented in our algid cases during that epidemic.†

Mr. Brown assures me that, after every consideration he feels convinced that the term "algid" is here perfectly applicable; and that the change to this state was *sudden*, as noticed by Maillot, who also observes that these algid symptoms are exclusively peculiar to fevers of a periodic type.

Having last year been in contact with Dr. Jameson, staff surgeon, who has had much experience in West Indian fever, that gentleman writes me to the following effect:—"During my residence in Jamaica, from 1834 to 1845, I have seen *several* cases of the algid variety of Yellow Fever." He also states that he witnessed recovery in one "case of algid variety of Yellow Fever, successfully treated by quinine at Kingston, Jamaica, about 1843-4—serjeant McHugh, then of 60th, afterwards 48th, now, I believe, an officer in a West India regiment: quantity given during the whole attack exceeded 700 grains."

I am of opinion that if Yellow Fever epidemics can be always closely investigated, algid cases are likely to be found much oftener than may be supposed; for I find, on consulting Arejula on the epidemics of Spain, that he frequently speaks of this cold variety of the disease.—See p. 168, where he mentions *the marble coldness* ("*el frío marmóreo*"), also pp. 160, 173, 259, and several others.

Dr. Lafuente in reference to the *cold* variety of Yellow Fever in Spain, gives symptoms of a sufficiently unmistakeable character in his account of what occurred at Medina-Sidonia in 1801. At page 28 *b* and 28 *c*, we find the word "*lipirico*" and "*lipiricos*" employed; which, if rendered "*Lipyria*," will bring us to the precise word employed by the most ancient authors on fever, in which privation of heat was a leading feature; afterwards employed by Torti, and recognized by Cullen.

## HISTORY OF THE DISEASE.

Previous to entering into details, it may be stated that a disease is here understood in which, along with other symptoms, yellowness of the skin, partial or general (though by no means constant), and, towards the fatal termination, vomiting of a black or dark-brown fluid, still less *constant*, occur. As it will be necessary to refer frequently to the yellow-fever epidemics of Spain, and as, notwithstanding all that has been written

\* This appearance mentioned as occurring at Cadiz, 1800. Arejula, p. 172.

† Cases of this kind had been noticed in previous years by Dr. Bobadilla.

upon the subject, the identity of yellow fever, in that country with the black-vomit fever of the West Indies, North and South America, has been denied, so late as 1828, by a French physician (Dr. Rochoux), who went to investigate the Barcelona epidemic of 1821, it may be proper here to premise that "*the perfect identity*" of the disease has been admitted\* to have been established beyond all doubt at Gibraltar in 1828.

Among writers on Yellow Fever, the names of respectable men will be found who maintain that this disease has made its appearance on the continent of America, in the West India islands, and certain parts of Europe, in modern times only. In opposition to this it has been shown, that though in former ages this disease may not have been observed to prevail epidemically in that part of the neighbourhood of the Mediterranean where Hippocrates practised, it is not the less true that this close observer had been familiar with a fever in which the two symptoms considered by most writers as characterizing the disease (yellowness of skin and black vomit) were sometimes present.

We are reminded by Humboldt that the period of the first description of a disease furnishes no evidence of its having only then for the first time appeared; and the *ensemble* of the symptoms of Yellow Fever being only to be found fully and accurately detailed by writers of the last century, will scarcely be considered as furnishing conclusive evidence of the non-existence of similar epidemics at periods more remote. Owing to the state of medicine in former ages, and to the fact of practitioners having been so few that the sick were not unfrequently wholly destitute of medical aid, the exact nature of many epidemics which reigned from time to time, under the names of "pest," "pestilential disease," "black death," "yellow death," &c., has not been handed down to us. We have a remarkable proof of this in epidemics which prevailed in this country formerly, under the name of "sweating sickness;" the accounts of that disease being quite unsatisfactory. To admit that all ancient epidemics within certain parallels of latitude, and termed pests or plagues, were of the character of true plague, while all the epidemic fevers of modern times, which have so frequently afflicted the inhabitants of the same latitudes, *have not* possessed the characters of plague, but those of Yellow Fever, would be admitting what is but little conformable to the usual course of nature. Père Dutertre, one of the oldest writers on the Yellow Fever of the West Indies, employs the term "*peste*," when detailing symptoms not corresponding with those of plague, but such as peculiarly belong to Yellow Fever.

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\* Vide *Bulletin des Sciences Medicales*, vol. xviii. p. 90 (1829). This identity seems to have been placed beyond all doubt by my having shown that we had, occasionally, at Gibraltar, the same kind of hæmorrhagic depôts or tumours which Dr. Keraudren and others had noticed in the Yellow Fever of the West Indies.

To give even a partial view of the arguments employed by various British writers in support of and against the statement of the Yellow Fever having been imported into the West India islands for the first time in 1793, would occupy more space than could with any propriety be devoted to the point. Its importation into the island of Grenada in that year rested chiefly on the authority of the late Dr. Chisholm, who believed that he had traced the origin of the fever to the ship "Hankey," which had lately arrived from the island of Bulam, situated on the west coast of Africa. This statement of the importation into the West Indies of a "*nova pestis*," as it was then called, has since given rise to much controversy; but those who consult Bancroft's *Essay on Yellow Fever*, and a small treatise on the disease published in 1818 by Dr. James Veitch, an experienced naval surgeon, will find details of a very interesting nature, which go to prove that on this occasion Dr. Chisholm had certainly proceeded on very erroneous data.

Respecting the alleged importation of the disease into Grenada by the "Hankey," I must refer to the observations of Dr. Cragie, of Edinburgh. This distinguished physician states to the following effect in his "*Practice of Physic*," vol. i.

"Dr. Chisholm maintained that the disease thus generated and imported was a new pestilential disease, unheard of before in the West Indies, entirely different from Yellow Fever and malignant pestilential fever; he specially applied the name of Bulam to it. The whole of this, however, originated in mis-statements of facts, and complete ignorance of the previous history of the West India fever.

"It may therefore be regarded as certain, that the disease which was thus, by a degree of ignorance and misapprehension almost unrivalled in the history of medicine, then and since denominated *Bulam* fever, was only the usual form of fever common, in certain seasons, to the whole of the West India islands, and which has prevailed there, chiefly among the newly-arrived or not long-resident Europeans, ever since commercial intercourse has been fully established between Europe and these islands." And, truly, it was unpardonable for Dr. Chisholm and his followers to apply the term "*Nova pestis*" to a disease long before distinctly recorded by a celebrated nosologist, SAUVAGE, and other writers, who should have been consulted. Without referring to old Spanish and French authors, Drs. Chisholm and Pym might have found, from at least the following writers, that there was nothing *new* in the matter:—Town, 1736; Lining, 1758; Hillary, 1766; Mackittrick, 1766; Lind, 1772; Hunter, 1788;\* Moseley, 1790.

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\* Sir W. Pym states, p. 51, second edition of his work, that the health of Jamaica previous to the arrival of the "Hankey" was good, and (among other authorities, all of whom are referred to in regard to periods *subsequent* to that about to be

Père Dutertre would seem to be the first by whom details of the symptoms and progress of this disease in the West India islands have been transmitted to us.\* From the remarkable muscular pains often felt by a patient labouring under an attack, as if from heavy blows, it was then called *coup de barre*; and Père Dutertre, considering it a new disease when he first saw it (1635), termed it "the pest unknown previously in these islands." He notices the yellowness of the skin particularly; and though he says that the disease was imported into the islands by "some ships," and in another page by a particular ship, "Le Bœuf," from Rochelle, he says that those "were chiefly attacked who were employed in clearing the land in different islands, and were exposed to the poisonous vapours and exhalations."†

Père Labat, on landing at Martinique in 1649, found the disease raging in that island, and the monks belonging to the convent of his order suffered severely. He tells us that he himself had the disease twice; that people were frequently attacked so suddenly and severely that they fell down in the streets; that hæmorrhages from the several natural orifices, and even from the skin occurred; and that the disease usually proved fatal within five, or six days. He states that the

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mentioned) quotes (p. 55) Dr. Hunter, respecting the salubrity of Fort Augusta and Stony Hill in 1781-2, in proof of his assertion.

On consulting "Observations on the Diseases of the Army in Jamaica," &c., by John Hunter, M.D., London, 1796, pp. 26 and 27, we find it is indeed true that that writer speaks favourably of the two above-mentioned localities; but they are expressly pointed out as exceptions, and by way of contrast with the sickly, nay deadly state, of other parts of the island during the period referred to,—which will abundantly appear from the following quotations:—

At p. 11, Dr. Hunter says, "Four regiments were sent from England in 1780 to Jamaica; they arrived there the 1st of August, and before the end of January ensuing, not quite six months, one-half of them nearly were dead, and a considerable part of the remainder were unfit for service."

At p. 57, he says, "An average of the number of sick during three years and a half, in which are included the convalescents, gives one-third of the army unfit for service at the time of the greatest sickness, and one-eighth at the time of the least sickness. The average of deaths annually upon the whole is nearly one in four, and of discharged men about one in eight, which, together, makes the loss three-eighths of the whole. In less than four years there died in the island of Jamaica 3,500 men; those that were discharged amounted to one-half of that number, which make in all 5,250 men lost to the service in that short period of time from the climate and other causes of mortality, without a man dying by the hands of the enemy."

Speaking of the symptoms of the disease, p. 64, he says, "The vomiting is sometimes constant and violent, especially in the worst kind of the disease, and the blood being frequently in a dissolved state is forced into the stomach and thrown up, forming, what has been called by the Spaniards, the *black vomit*."

Is it then credible that with this book before him the British Superintendent of Quarantine should write the following, at p. 59 of his second edition?

"These quotations, from the highest authority, prove that for several years before 1793 there was no instance of that species of disease prevailing epidemically in the West Indies, which is characterized by black vomiting," &c.

\* *Histoire Générale des Antilles*.

† *Ibid.*, p. 81, ed. in 4to.

disease was called *maladie de Siam*, from the belief of its having been imported into Martinique by a ship of war, the "Oriflamme," "which, coming from Siam with the remains of the hospital establishments which had been at Mergay and Bancock, touched at Brazil, where she became infected with the disease which reigned there for seven or eight years."\* This account of the introduction of the disease into Martinique relates to the year 1688, being some years before his arrival in that island; and his statement would seem to rest altogether on the *belief then prevalent* as to the circumstances.

At page 337 of Dr. Baneroft's *Essay on Yellow Fever*, we have evidence of the existence of the disease at St. Domingo in the year 1731; and, in subsequent pages, of its having prevailed there epidemically in 1733, 1734, 1739, 1740, 1741, and 1743.

The insalubrity of that island was manifested soon after its discovery; for it appears that the sickness among his men gave Columbus great anxiety. It could scarcely have been expected that anything very precise as to the nature of the disease from which they suffered should have been transmitted to us. A late historian, Washington Irving, merely informs us that "when they fell ill their case soon became hopeless." Reasonable inferences may however be drawn from passages in old Spanish historians. Oviedo, in his *Historia General de las Indias*,† speaks of a great mortality among Columbus's people in 1494, which he attributes to the humidity of the island. He says that those who returned to Spain were of a yellow or "saffron colour;" that people finding the country so unhealthy objected to go there;‡ and that in consequence 300 convicts were at one time sent to St. Domingo. He adds, that if the king offered him the Indies he would not go there. M. Moreau de Jonnés cites§ one or two other passages from Oviedo on the same subject, which I have not been able to verify by a reference to the edition within my reach. Further details are given by Herrera (Madrid 1601), as to the violence, suddenness of attack, &c., of the disease which carried off so many of Columbus's men in St. Domingo; and he refers to a letter|| written in 1498 by Columbus to the king of Spain, attributing the sickness of his men on their first arrival to peculiarities in the air and water.

Respecting the accounts of the existence of the Yellow Fever at remote periods on the American Continent, it would appear that Dr. Fournier Pascay, of Paris, who for several years

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\* Nouveau Voyage aux Iles de l'Amerique, tome 1.

† Ed. in folio 1547, book ii., cap. 13.

‡ Book iii., cap. 4.

§ Monographie de la Fièvre jaune.

Book iii., e. 15.

devoted much attention to all questions connected with the disease, considers it identical with that referred to by Ferreira da Rosa, in his account of Pernambuco, printed at Lisbon in 1649. In the beginning of the last century the disease, from its appearance in various parts of Spanish America, under the name of *vomito prieto*, attracted much attention; and it is particularly referred to by the historian Ulloa, who resided for some years in that country. The word *prieto*, it may be remarked, is the Portuguese or nearly obsolete Spanish term for *black*: in Spain the word *negro* is now universally substituted. A small pamphlet of 62 pages, by a Dr. Gastelbondo, written at Carthagena (S.A.) in 1753, and printed at Madrid in 1755, was probably the first work *ex professo* on the black-vomit fever, as it appeared in South America; he gives his experience of the disease during 40 years; says, in the title-page, that he is about to write on a disease of frequent occurrence in that part of the world; mentions change of climate and mode of living among the causes of the disease in new comers; and says, that the natives of Carthagena, Vera-Cruz, &c., were not subject to attacks of the true black-vomit fever, though liable to the "*chapetonada*," a disease resembling it in some respects.

In North America the prevalence of Yellow Fever epidemically, previous to 1793, seems unquestionable; and authorities may be cited for its appearance at Boston in 1693; at Philadelphia in 1695, 1741, 1751, and 1762; at Charlestown in 1695, 1732, 1739, 1745, and 1768; at New York in 1702; and in Virginia in 1744.

We come now to the history of Yellow Fever in that part of Europe where its frequent appearance, within the last half century, has so justly excited the attention of the profession, and of those governments who rank the investigation of such subjects among their first duties. Some writers (among whom was Sir Gilbert Blanc) have stated that the first appearance of Yellow Fever in Spain was at Cadiz in 1764; next in 1800 in the same city; and at Malaga, for the first time, in 1803.

It seems strange that, with respect to Cadiz, those writers should have overlooked the remarkable epidemics at that place in the years 1730, 1731, and 1736, recorded by different authorities, the two first being very particularly noticed by Villalba, in his curious work "*Epidemiologia Española*." It seems equally extraordinary that those writers should have overlooked the black-vomit epidemic which prevailed at Malaga in 1741, described by Dr. Rexano, and since frequently referred to. With respect to epidemics which prevailed in Spain, previous to those of Cadiz and Malaga, many consider the evidence imperfect as to the disease having been, in any of them, similar in character to that under consideration; for

in those days all epidemics causing great mortality were called "pests," or "pestilential diseases." In epidemics called pests, recorded as having prevailed at Malaga in 1678 and 1679, two physicians, Drs. Checa and Molina, sent officially to inquire into the nature of the disease, pronounced it *not* plague. The writings of Spanish medical men being but little known to the profession at large, quotations from some of them on the present subject may be the more admissible.

Dr. Hurtado, of Madrid,\* adduces proofs in support of the prevalence of the disease epidemically in former ages in Spain. He quotes Dr. Gareia Suelto as being of his opinion that such epidemics appeared at periods much more remote than 1730, and cites him as stating that "the most distinguished men of the profession move as it were in a career new to them, but long known to Spaniards their countrymen. If the medical history of Spain had been more familiar to them they would have availed themselves of the excellent descriptions and important illustrations to be found in the work of Antonio Fonseca, on *the peste* and contagious diseases, and on the epidemic fever of 1621." Hurtado also quotes Sebastian Nuñez, Pablo Correa, Manuel de la Cerda, and others.

The frequent application of the word *atrabilis* in ancient times to any dark fluid ejected from the stomach, tended, no doubt, to create obscurity as to the character of diseases; and in Spain, medical men, for want of a better name, sometimes employed the words *fiebre dudosa* (fever of a doubtful nature) when speaking of the epidemic disease. Escobar is quoted by Villalba respecting an epidemic which prevailed in Carthage in the autumn of 1648; which was attributed to local causes. Escobar states that in his time the endemic fevers of Carthage and Alicante sometimes became *pestilential* in the autumnal months. It appears from Villalba's work, that in 1648 other towns besides Carthage, as Cadiz, Seville, Alicante, and Valencia, were afflicted by the epidemic; and it is remarkable that some of the writers of that period state that the disease *was carried to the West Indies* from one of those towns, from whence it was again brought back to Spain, and commenced fresh ravages at Barcelona, Gerona, Tortosa, and "almost in every town in Catalonia." From this we may at least infer that the identity of the Spanish and West India diseases was *then* acknowledged.

According to Villalba, three formidable epidemics took place in remote times at Barcelona within a period of eighteen years; one in 1497, another in 1501, and the third in 1515; and as they prevailed in the summer or autumnal months, their iden-

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\* This writer, like many other medicos, was not at liberty, previous to the declaration of the constitution in Spain in 1821, to publish his opinions openly on these matters.

tity with the modern epidemics of Spain has been inferred. Villalba records an epidemic at Barcelona in 1589, which lasted from June to December—the deaths up to the 20th October having been 10,935. On this occasion the resident physicians of Barcelona maintained that the disease *was not plague*.

A Dr. Porecl wrote, in 1565, on an epidemic which prevailed at Saragossa in the preceding year, and which ended in the month of December. He states that the symptoms were sometimes very insidious; that the patient seemed to be going on well,—pulse natural, skin temperate, &c.—till the fourth day, when the countenance became altered, and faintings took place, followed commonly by death: he adds that sleeplessness, extreme anxiety, (the patient rolling about the bed,) peculiar pain in the region of the stomach, and vomiting of a fluid (which he calls *colera*) of various shades of colour, took place. He notes, moreover, that the countenance became livid and yellow, (*livido y amarillo*.)

The work of a Dr. Andosilla is also cited, in which he speaks of a disease under the name of *peste*, which prevailed in some Spanish towns in the *autumn* of 1600. He visited those towns officially, and describes the disease as not having the characters of plague, but others “new to him.” In 1649, a Dr. Morillo, who had been employed at Marbella and other towns in Andalusia during an epidemic, went also to Gibraltar, to witness an epidemic, which, according to an old Spanish history of Gibraltar, by Ayola, proved so fatal, that the people, losing all confidence in human means, instituted processions to the neighbouring hermitage of St. Roque, which were kept up annually in the month of August, till the surrender of the garrison to the British in 1704.

There is a record of that garrison having, in the autumn of 1727, lost 500 men by fever, but the character of the disease is not described. By a document in my possession from the late W. Hill, deputy inspector-general of hospitals, dated London, June 13, 1832, it appears that in 1798 the 48th regiment, arrived in Gibraltar from England, and that soon after, a severe fever appeared among the men, which carried off about 100 of them. This fever, which he says was confined to the recruits, of which there were great numbers, “Dr. Harness, then physician to Lord St. Vincent’s fleet, and afterwards one of the commissioners to the Siek and Hurt Board, declared to be *precisely the same he had seen in the West Indies*.”\* In Trotter’s *Medicina Nautica* it is stated that 257 deaths from fever took place in the above garrison in 1800, among the military; the average annual mortality among the military there having been only 38.

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\* Addenda (A.), p. 206.

With respect to Gibraltar, therefore, these facts may be considered sufficient to establish that, previous to the Yellow Fever epidemic of 1804, the disease had made its appearance there to a formidable extent: indeed, it is well known that, along that part of the Spanish coast, no other form of epidemic proves so fatal. It may be added, that Dr. Monro says in his work on the diseases of armies, that in 1799 a fever made its appearance at Gibraltar, which he considered similar to that of the West Indies.

It is probable that the existence of the *causes* of Yellow Fever have not been confined to the localities in Spain recorded as having been visited by the disease; those causes may equally have been present, at numberless other places, though not recognised, in consequence of such places being uninhabited. We cannot always be sure of the existence of agreeable localities until the presence of inhabitants establish this.

Respecting Yellow-Fever epidemics in other parts of Europe, that described by Palloni, Tomassini, and others, as having taken place at Leghorn in 1804, is the most remarkable. We have an account, by a Dr. Kennedy, of an epidemic at Lisbon in 1736; and, from the symptoms, there seems little reason to doubt of the identity of the disease with Yellow Fever. Professor Salva, of Barcelona, considers a fever with yellow skin which prevailed very extensively in a district of the Canton of Berne, during a period of very extraordinary heat, in the year 1762, and of which there is some account in a volume of the proceedings of the French Academy of Sciences for 1763, as similar to the Yellow Fever of Spain. Some recognise this disease also in the fever with yellow skin, hæmorrhages, &c., described by Frank as occurring in Hungary.

The existence of this fever occasionally at points higher up the Mediterranean than those already mentioned, rests upon respectable authority. The late Dr. Alexander, surgeon to H. M. forces, who had ample experience of Yellow Fever in the West Indies, declared to me on a particular occasion, that he witnessed many deaths from the disease in Sicily, soon after the return of Sir John Stewart's army from Lower Calabria in 1806; and that while at this latter place, some cases occurred among our troops. The existence of sporadic or scattered cases of Yellow Fever with black vomit even in England and France, has been insisted upon by some persons; those referred to in the *Dictionnaire de Médecine*, vol. xxi. p. 17, as having occurred at Paris in the hot summer of 1822, seem most worthy of attention.

In turning to East India Records, the mention at p. 46 of the Bengal reports on cholera, of a fever with yellow skin which occurred in that presidency in 1816, can hardly be held as conclusive. But we find, in a memoir by Mr. Walsh, of the

medical department of our army during the Burmese war, that that gentleman, while in charge of cases of the fever prevalent in the army, was surprised by the sudden appearance of some with black vomit and yellow skin. It is recorded, as has been noticed by Dr. Johnson, in his work on tropical climates, that those symptoms appeared in a fever which prevailed in the hospitals at the Isle of Edam, appropriated for the sick of the force employed for the reduction of Java in 1811.

At Sierra Leone this disease is recorded as having occurred, to a remarkable extent, in the year 1823.

### SPORADICS.

Connected with the history of Yellow Fever, it is always considered a point of very great importance to ascertain whether it be a fact that, in those parts of the world where the disease is observed to prevail epidemically, single, or scattered, ('*sporadic*') cases have occurred in ordinary years. To the mass of evidence on this point, from the West Indies and America, not one word need be added in confirmation of the fact. In Europe a few only hold out against this statement; in England, probably not more than two or three. But of late years so much attention has been paid to the subject that unless people be prepared to prove that symptoms, grouped together in a certain order, in conjunction with similar *post-mortem* appearances, do not always constitute the same disease, it is quite idle longer to dispute the point. Among many other French physicians who have paid great attention to the subject of Yellow Fever, and who have recognised the existence of sporadic cases in Spain, are Drs. Pariset and Robert, leaders of the contagionists. In Spain we find Dr. Arejula, a leading contagionist, and Dr. Flores Moreno, also a contagionist, admitting it freely in their works; besides Drs. Piguille, Salva, and several other men of note. I am in possession of such a body of evidence, drawn from registers and other authentic sources at Gibraltar, as would of itself place the matter beyond all doubt. In the month of April, 1829, the records of the civil hospital in that garrison were examined at my suggestion, and a certificate drawn up and signed by nine gentlemen, to the following effect:—

*Gibraltar, 13th April, 1829.*

"We, the undersigned, have this day heard the foregoing thirty-nine cases, which have been extracted and condensed from the records of the Civil Hospital, carefully read over, and which have been compared with the original text.

We are unanimously of opinion, that, with the exception of case No. 23 (Samuel Bird), they are identical with the cases of

the epidemic fever which existed in this garrison during the latter part of the year 1828.

(Signed) JOHN GILLICE, (a)  
 Assistant Surgeon 12th Regiment.  
 A. BROWNE, M.D. (e)  
 Assistant Surgeon 23rd Regiment W. F.  
 EDWARD DOW, (b)  
 Acting Deputy Inspector of Hospitals.  
 HUGH FRASER, (a)  
 Surgeon of Civil Hospital.  
 GEORGE BROWN, (a)  
 Assistant Surgeon 43rd Regiment.  
 J. MILLAR, (d)  
 Assistant Staff Surgeon.  
 J. GILLKREST, M.D., (b)  
 Surgeon 43rd Regiment.  
 R. AMIEL, (c)  
 Surgeon 12th Regiment.  
 CHERVIN, D.M. (f)"

(a) The gentlemen marked thus had been here during the whole of the epidemic.

(b) These gentlemen had witnessed epidemics in the West Indies, as well as at Gibraltar.

(c) This gentleman had witnessed three epidemics in Gibraltar,

(d) This gentleman had seen the Yellow Fever, or Bulam of Dr. Pym, in the West Indies.

(e) This gentleman had seen the Yellow Fever at Gibraltar during a part of the epidemic season of 1828.

(f) Dr. Chervin had made the Yellow Fever his particular study for fifteen years previous to 1829, in various parts of the world, and had seen more Yellow Fever than any person then living.

There is also evidence in a report drawn up for the Transport Board, by Dr. Gray, formerly physician to the Mediterranean fleet, and for some years physician to the Naval Hospital, Gibraltar. After stating that "remittents" (with dark stools, hæmorrhage from the mouth, nose, &c.) are more or less endemic at Gibraltar, he says:—

"I have also observed, in some instances, matter ejected from the stomach, of a dark colour, resembling the grounds of coffee. During the long period of my serving, I cannot recall to my recollection a single instance of fevers being communicated from one person to another, either amongst the medical attendants or nurses."

And in a document (published in the second edition of Sir W. Burnett's work) by Staff-Surgeon Glasse, who had served ten years in Gibraltar, and witnessed the epidemics of 1804 and 1813, as well as the cases of Yellow Fever which occurred in 1810. &c. ; he says:—

"During the autumn I have been in the habit of seeing solitary cases of fever, attended with black vomiting and other severe symptoms, both in the town and "South" (see plan T),

without the disease being communicated to others confined in the same building."

In a letter to Sir James M'Grigor, Dr. Thomas Smith, Surgeon, 23rd Regiment, reports on a sporadic case of Yellow Fever occurring in Gibraltar in March 1830; fearing that so marked a case might not otherwise obtain the attention of the Director-General of Hospitals in England.

After detailing other symptoms, Dr. Smith expressly refers to the occurrence of the yellow skin and "black vomit:" also states that hæmorrhages, suppression of urine, hiccough, disturbance of the cerebral functions with jactitation took place. The man died on the fourth day after admission.

It is quite certain that for the last 20 years strongly marked sporadic cases of this disease have been of extremely rare occurrence at Gibraltar, while at the same time the existence of sporadic cases of true Yellow Fever within that time even in London and Paris rests upon excellent authority.

### CONTAGION.

Considering the misconceptions which so often arise regarding the words *Infection* and *Contagion*; I think it proper to explain the sense in which they are here used.

In conformity, I believe, with the sense in which it is employed by medical writers in France, as well as in other continental nations, I mean by contagion, the power of communicating disease, from the person of an individual labouring under it, to that of another,—whether by actual contact,—(the *immediate* contagion of the French); through the medium of linen, bedding, or any substance whatever;\* through the medium of the air, in a room in which an individual labours under the disease; the capability of being propagated by means of excretions or exhalations from the bodies of those affected; or, finally, by means of exhalations from the bodies of those who have died of it.

The transmission of a disease in any of the above ways being clearly demonstrated; its capability of subsequent extension, or general dissemination, may be reasonably contended for.

By *infection* I would be understood to mean, (as I also think the French do) a principle which produces the disease, depending altogether upon *local* causes, and having no relation to emanations from persons labouring under the disease, or from the bodies of those who have died of it.

Thus I would call a malaria spot, or filthy locality, a *focus of infection*.

The discussions regarding the contagion or non-contagion of Yellow Fever have been of ancient origin, and are still con-

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\* This and following are the *mediate* of the French and other authorities.

tinued. Indeed invitations have been held forth within the last few years, by writers who feel deeply interested in this important disease, that those who have had much experience on the subject, "should not allow it to remain in its present state."

Gibraltar being in every way a station of high importance to the British Government, and my services there in responsible situations having extended over many years, I trust that my statements will be found given with all the care and all the fidelity which the importance of such a subject demands. The interest I have so long felt in everything connected with the health of that garrison, naturally induces me to give the Yellow Fever epidemics, which have broken out there, the first place in my consideration.

The calamity of an epidemic at Gibraltar, affords greater facilities for the establishment of truth, as to its origin and progress, than perhaps any other place whatever.

Its surface is very limited, it is under strict military government and police, affording all the means, *when so wished by the authorities*, for the most scrutinising investigation; and the population (civil and military, about 22,000,) not sufficient to embarrass the ready attainment of facts, yet large enough, to give all the materials necessary for drawing solid conclusions.

Under these circumstances ample opportunities are afforded for the detection of erroneous statements.

I have before referred to what I consider as the first recorded epidemic at Gibraltar in 1649, which I believe to have been Yellow Fever, from the mortality it occasioned, and from its originating in the month of August. Since its occupation by the British in 1704, the following visitations of Yellow Fever have occurred in Gibraltar.

## 1804.

The severest on record (commencing in August), when, in a population of about 15,000, the following number of deaths took place:—

Military	.	.	.	.	869
Civilians	.	.	.	.	4864
					<hr/>
					5733

## 1810.

(Scattered or sporadic cases.)

Military	.	.	.	.	6
Civilians	.	.	.	.	17
					<hr/>
					23

1813.

Military . . . . .	391
Civilians . . . . .	508
	<hr/>
	899

(Between the 8th of September and the 3rd of December.)

1814.

Military . . . . .	114
Civilians . . . . .	132
	<hr/>
	246

From August to November.

1828.

Military . . . . .	507
Civilians . . . . .	1170
	<hr/>
	1677

From August, to the 14th of January 1829, when the last death took place from Yellow Fever.

In nearly all the foregoing, it will be observed that the disease commenced in or about the month of August, as is found to be the case in the great majority of Yellow Fever epidemics throughout Spain.\*

It would be impossible to enter fully into the numberless details connected with the origin and progress of the Yellow Fever at Gibraltar in 1804, 1810, 1813, and 1814.

But in the works of Sir William Burnett, Director General Navy Medical Department,—the late Dr. Bancroft,† Deputy Inspector General of Hospitals,—Mr. Amiel,‡ formerly on the Staff, and attached for some years to the civil poor of some of the districts of Gibraltar, afterwards surgeon of the 12th Regiment in the garrison, and, lastly, many years surgeon to the Civil Hospital, who was in fact the chronicler of all the diseases of the Rock for nearly 40 years,—and Dr. O'Halloran,§ formerly of the 64th Regiment,—most ample accounts are furnished, (especially by Dr. Bancroft, in his *Sequel to his Essay on Yellow Fever*,) showing the very wrongful efforts of

\* “The epidemic which reigned in Andalusia in 1804, commenced in the month of August in ten, and in September in eight of the twenty-three towns in which it prevailed that year.”—*From Barcelona Manifesto of 15 Physicians (Maclean's Evils of Quarantine Laws, p. 131.)*

† “*Essay on Yellow Fever*,” and “*Sequel to Essay on Yellow Fever*.”

‡ *Memoir on Yellow Fever*.—Edinburgh Med. and Sur. Journal, April, 1831.

§ On the Yellow Fever of the South and East Coasts of Spain.

persons with exaggerated views of the utility of quarantine, to prove the disease of a highly contagious nature.

It will therefore be indispensable that those who desire to obtain a correct view of the occurrences which took place in those years regarding Yellow Fever, should make themselves familiar with the writings of the above mentioned authors, many of whose statements I had an opportunity of verifying after my arrival in 1822. I have been assured by Mr. Amiel, that the impression given\* by the present superintendent of Quarantine in this country, as to the disease having been cut short in 1810 by his recommendation of segregating the eases, is utterly fallacious; for, as has also been specially certified to me by the Signor Bobadilla, a respectable practitioner resident during many years in Gibraltar, the progress of the disease was stopped "*as it is always found to be,*" by the setting in of a cold wind from the north.†

It must be obvious that placing a point like this on its true footing is of the highest importance. Notwithstanding the body of evidence on record against contagion in all the Gibraltar Yellow Fever epidemics up to the year 1814, and though from Bancroft's works it appears that among the medical men of that garrison the majority of opinions had been greatly against it, it was nevertheless natural that unbiassed persons should become influenced by the statements published by two officers of the Quarantine Department,‡ who, having been on the spot, had ample opportunities of arriving at the truth. The circumstance here alluded to is the assertion that, during the epidemic of 1813, the people employed in the dock-yard having been strictly separated from the rest of the garrison, remained free from the disease. Here, then, was, *if true*, evidence in support of the utility of quarantine, and of the propriety of separating the healthy from the sick. But what was the astonishment of the profession on finding that MIS-STATEMENTS HAD HERE TAKEN THE PLACE OF FACTS, as shown by Dr. O'Halloran. During my residence at Gibraltar, I had ample means, by referring to the official authorities at the dock-yard, of confirming the assertion of Dr. O'Halloran that several cases of the fever prevalent in 1813 occurred there, as well as some deaths; indeed the names of twenty-three (of which seven proved fatal) are given in another place,|| so that no impressions favourable to the accuracy or candour of the Quarantine officers can be entertained; and in the justly severe remarks of Dr. O'Halloran

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\* See Pym on the "Bulam" Fever.

† See Addenda (B.), p. 209.

‡ Sir William Pym,—and Inspector W. W. Fraser: the latter in a published letter to Lord Chatham, the then Governor. Mr. Fraser was afterwards appointed Quarantine Officer in Liverpool.

|| See Addenda (C.), p. 210.

to which no reply has been made, future observers of circumstances connected with the public interest, as well as the medical officers of the navy and army to whom the Superintendent of Quarantine has dedicated his work, have received a salutary warning against a too hasty credulity.

### GIBRALTAR EPIDEMIC OF 1828.

I shall now refer to the facts bearing on contagion, as they occurred during the epidemic in 1828, when it fell to my lot to observe its rise, progress, and termination. At the commencement of the epidemic there were very few medical men in the garrison who could be called anti-contagionists. Conceiving that our medical chief, the late Dr. Hennen, was disposed to make up his mind, perhaps too soon, against importation and contagion, Dr. Thomas Smith, late surgeon of the 23rd Regiment, and myself, wrote to request that he would give further attention to the reports regarding the importation of the disease by a Swedish ship from the Havannah, called the *Dygden*;<sup>\*</sup> but an impartial consideration of all the facts which passed in evidence before us subsequently, left no doubts as to the cause, being of a strictly local nature. At the strangely constituted Board appointed to inquire into the origin of the disease, much passed, over which a veil has too long been suffered to hang.

On the cessation of the epidemic, a despatch was forwarded to the Governor of Gibraltar from the late Sir George Murray, then Secretary of State for the Colonies, directing a commission of inquiry for the purpose of establishing the origin of the disease, and to decide on the question of its alleged importation, at which he himself, the Governor, was to preside. †

Not finding himself equal to the task, however, the Governor, with the sanction of the Secretary of State, appointed (to the astonishment of all who had read the works of Dr. Bancroft and Sir William Burnett) Dr. Pym, British Superintendent of Quarantine, to take his place. Thus in fact, creating that gentleman a judge presiding in his own cause, and converting, as my readers will perceive, an investigation on a most important subject, *into a mere mockery*.

The Board was composed of the following gentlemen:—

#### *President.*

Dr., now Sir WILLIAM PYM.

#### *Members.*

T. JONES HOWELL, Esq., Judge Advocate, and Judge of the Vice-Admiralty Court.

<sup>\*</sup> See Addenda (D.), p. 212.

† "As the object of the proposed investigation is merely to ascertain a fact, and may therefore be more properly accomplished by the careful examination of impar-

Colonel CHAPMAN, Royal Engineers, Colonial or Civil Secretary, &c.

Lieut.-Colonel FALLA, Town Major, as such, head of the Police, and therefore responsible for the sanitary condition of the town, state of the population, admission of strangers, &c.

WILLIAM SWEETLAND, Esq., for many years Captain of the Port, and, as Pratique Master, head of the Quarantine Department.

Dr. BROADFOOT, principal medical officer, and for several years of the Quarantine Department at Corfu and Gibraltar.

Dr. BARRY, Staff Surgeon, Secretary to three Commissions connected with Yellow Fever; named for this occasion Vice Inspector of Health.

The following is a summary of the opinions given by the President and members of the Board:—

The PRESIDENT.—“From the very strong evidence before the Board of the first person attacked by the late epidemic fever, having been connected with the shipping, I am of opinion that the disease was of foreign origin; and that neither local nor atmospheric causes have had any share in its production; Gibraltar being one of the most healthy spots in Europe, no soldiers of the garrison having died of fever during the months of August, September, and October of 1827, and only 19 soldiers having died from fever in the course of the five years preceeding the year 1828, during the sickly months of August, September, and October.”\*

Judge HOWELL.—“Upon a careful review of all the proceedings before this Board, I am of opinion that the evidence brought forward has totally failed to prove that the late epidemic disease was introduced from any foreign source, either by the Swedish ship Dygden, or by any other means; and I am further of opinion that the late epidemic had its origin in Gibraltar.”

Colonel CHAPMAN.—“Judging from the evidence produced before the Board, the manner in which it has been given, together with the description of persons who have been brought forward as witnesses, I am decidedly of opinion that the late epidemic disease was of local origin. As to the importation of

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tial evidence, than by the application of scientific research, I recommend that the inquiry be entrusted to a Board to be presided by yourself.”—*Extract of a Letter from Sir George Murray to Sir George Don, 31st October, 1828.*

\* What rational inductions we have here!! The following is another specimen of contagionist reasoning:—“During the time that the disease had been going on on board the transports in the bay, the garrison continued in perfect health until the 20th of October [1810], when, in consequence (as I must suppose) of a breach of Quarantine regulations, which, however, could not be detected, a Minorcan family in the South District, belonging to the dockyard, was attacked with the disease.”—P. 49, 1st Edit.; 29, 2nd Edit.—*Observations upon Bulam Fever, by Sir William Pym.*

the late epidemic, I am of opinion that the attempts to prove the introduction of the disease, after months of previous inquiry, by those who wished to prove it, have wholly failed."

Mr. SWEETLAND.—"After the most attentive consideration of the evidence which has been brought forward, I have discovered nothing which has carried conviction to my mind as to the cause or origin of the late epidemic fever. On the one hand, it has not been shown, that any of the causes stated in support of the doctrine of its being of domestic origin, existed in a higher degree in the year 1828, than in many preceding years, when the garrison was free from that disease; and on the other hand, no vessels have arrived during the last summer having the Yellow Fever on board, nor has that disease discovered itself among any of the shipping in the port. In the absence, therefore, of any proof on either side, I must decline hazarding an opinion on a subject which has hitherto baffled the researches of the most learned physicians of all countries."

Lieut.-Colonel FALLA.—"I am of opinion that the late epidemic was not of local origin; but, from the strong presumptive evidence before the Board, that it was introduced."

Dr. BROADFOOT.—Opinion, at great length, may be considered literally both for importation;—and local origin.

Dr. BARRY.—"I am of opinion that the late epidemic was not of local origin, and was imported."

With regard to the statements of some of the members of the Board, concerning the condition of the town, I was much struck, on my first arrival, with the general cleanliness of the streets; but my observations, after a settled residence, convinced me that, in numberless localities, its sanitary condition was exceedingly imperfect, notwithstanding what appeared to be the great care taken by the authorities to prevent accumulations unfavourable to health.

During the summer season, the sewers necessarily retain a quantity of filth, till carried off by the heavy periodical rains, on the recurrence of which their cleansing entirely depends at all seasons of the year. No summer passes without offensive smells emanating from those sewers, both within the walls of the town, and about the barracks and houses in the district called "South,"\* especially during the eddying gusts of an easterly,—or a direct westerly wind.

Cesspools are numerous, the town being but partially supplied with water-closets, and these having often an insufficient quantity of water, whilst many of the drains are constructed without a proper fall. It has been established, by the investigations which took place in the autumn of 1828, that, in the districts within the walls in which the fever first prevailed

\* See Plan, figure 8.

(Nos. 24 and 25\*), there was a greater accumulation of filth than ordinary in some of the branches of the sewers, and in the soil-pits. I am of opinion that when certain conditional causes, or more properly conditional states, exist, which favour, by their fortuitous assemblage, the emanation from the earth of the peculiar poison productive of Yellow Fever, a more accelerated developement of that poison is likely to take place near such places; and, in this view, foul sewers may fairly be considered as accessories.† To deny the influence of localities (as the neighbourhood of drains, gullies, &c.) as co-operating, in the development of the disease, or even in giving it a higher degree of malignancy, would be excluding facts which, in the minds of impartial people, must be quite conclusive.

Objections may, beyond all doubt, be made against the manner in which a great part of the town is constructed; many places inhabited by the poorer classes being excavated or scarped, to make room for the buildings, which, range after range, ascend for a considerable way up. Many of the houses have, therefore, from their being thus built on the side of the rock, necessarily most inadequate ventilation.

A common feature of the dwellings is that of small "*patios*," or square confined courts, entered by narrow alleys, in which the houses consist of several floors, each occupied by many families.

Gibraltar is therefore to be considered as greatly overcrowded, which may perhaps be conceived from a reference to the *view* attached to this report, showing the small space occupied by the town, near the northern extremity of the rock.

It was not until I had become principal medical officer,‡ in 1833, when I considered it my duty, as general superintendent of the health of the population, to visit, all the lanes, alleys, and "*patios*," that I could have formed any idea of the densely crowded and badly ventilated houses of these localities, and of the difficulty of conceiving how such places should in general prove healthy, amongst a mixed population of about 15,000, exclusive of military, within the walls.

For here we have, in the inadequate supply of water,—the existence of cesspools and ill-constructed drains,—and the overcrowding,—precisely the same assemblage of predisposing or localizing causes which are found to exist in the fever nests of London and other large towns, and which are so prejudicial during the prevalence of epidemic diseases.

In reference to the first appearance of Yellow Fever in

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\* Plan (A.)

† See Dr. Hennen's Letter to Sir G. Don, Addenda (E.), p. 212.

‡ Soon after my appointment it was ordered by the Colonial Office, without any communication from me, that the principal medical officer should also act in the capacity of Medical Member of the Quarantine Board.

1828, it is perfectly established, that the late Dr. Hennen, so early in the season as the end of July, visited a woman living in the district in which the disease first became prevalent,\* who, according to that gentleman's statement to Mr. Woods, then assistant surgeon, 94th regiment, laboured under the purest form of true Yellow Fever: this woman died.

It may be readily conceived, that in a disease where the symptoms are sometimes very slight, it was not easy to ascertain who were actually the first attacked, but 29 cases were generally admitted as having occurred up to the 1st September, in the southern and hottest parts of the town.†

On the appearance of the first cases of the fever, vague reports were freely circulated, that the disease was imported by the Swedish ship "Dygden."

The first proof, against its having been so imported, was contained in a document which I had seen from Mr. Sweetland, the Captain of the Port, from which it was clear that no person on board this ship could have communicated the fever to any one on shore, on the simple principle, that we cannot transmit to others that which we have not ourselves.‡

Immediately on the decline of the epidemic, I prepared a lengthened statement concerning Yellow Fever, in which I entered into every detail connected with the first appearance of the disease at that time in Gibraltar, as well as its progress.

This manuscript I have laid before different persons in high authority, and it was my intention to embody in this report much, if not all of the evidence I had collected, with a view to show the unfounded nature of the statements as to the importation and contagion of the disease in the garrison. But circumstances have enabled me to refer the reader to a clear exposition of all the proceedings on that occasion, conveyed in a manner which will render them more conclusive, and in every way more satisfactory.

I have furnished the several opinions of the members of the Board before alluded to; after due consideration of which by Sir George Murray, that acute observer saw fit to require from Mr. Howell, the only member of the legal profession on the Board, his reasons for not being in favour of importation or contagion. That gentleman gave a most elaborate reply, a copy of which will be found (p. 245) Appendix No. II.

Conclusions similar to those expressed in that document, had been placed on record by me in my details of the progress of the epidemic;—and it always appeared most extraordinary, and unjustifiable, that on this Board of inquiry which was intended by the Secretary of State to be so beneficial to the

\* District 24. Plan, Letter A.

† 24 and 25 Districts on Plan.

‡ See Addenda (F.), p. 214.

interests of humanity, the Superintendent of Quarantine as president, should have assumed the right in several instances of selecting the witnesses, which obviously prejudiced the question, and by which much of the truth was intercepted.

Several medical officers of the garrison who had much experience respecting the progress of the epidemic, were either not examined at all, or only in a very imperfect manner. I was among the latter, being surgeon to the 43rd Regiment, and present during the whole epidemic. After a very limited examination, I officially informed the President, by letter, that I had much to state; but like others, I was not called afterwards.

From what I felt due to the service of which I had been a member for so many years, as well as the cause of truth, I was induced to protest against such proceedings, which protest will, I presume, be found with the documents connected with the inquiry forwarded from Gibraltar to the Colonial Office.

In addition to Mr. Howell's observations upon the evidence of the witness Catalina Fenic, I think it well to state, that this woman having been cited on the 14th November, 1829, before a public Notary at Gibraltar, deposed as to the falsehood of the report that her children had visited the ship "Dygden."\*

I may mention that on my arrival in the West Indies in 1801, the progress of Yellow Fever, to a frightful extent, in Martinique and Dominique, among the newly arrived regiments *only*,† did not fail to convince me that all those under whom I was acting, were right in not considering it a contagious disease; the idea was never harboured, nor ever discussed, so plain and obvious were the facts.‡

With regard to the Gibraltar epidemic of 1828, I am able to state, that in the 43rd regiment, the admissions of Yellow Fever cases commenced on the 12th September, and though, between that day and the 29th September, 43 cases were treated, no hospital attendant was taken ill. The first day an hospital servant was attacked was on the 30th September, his employment that of cook, and his duties unconnected with the wards or the sick; he slept in a kitchen. The waterman fell ill whilst following his employment, that of bringing water from a distance, and his duties never took him into the wards. It was not till this period, that the southern district, in which there are about 173 houses,§ and where the hospital, containing the sick of six regiments, was situated,|| became affected by the noxious emanations which prevailed. What took place

\* See Addenda (H.), p. 223.

† See Appendix No. IV, p. 381.

‡ "To my great astonishment," says M. Dariste, who had been in the West Indies a great many years, "I found, on my return to my country, that the question as to the contagious property of the Yellow Fever was warmly agitated."

§ Plan, Figs. 8, 8, 8.

|| Plan, Letter (D.)

respecting the Orderlies of the 43rd, was also observed in the other regiments; and with all the medical officers, who had had no previous means of judging, this was a striking circumstance calculated to inspire confidence. Those of our permanent orderlies who suffered, were, for a month, in close contact, by day and night, with the patients, before being attacked. I was informed by Mr. Martin, surgeon 73rd, that, after his hospital was moved to the barrack at Windmill Hill,\* *he had no hospital servant taken ill.*

The same took place in the 94th, under medical charge of assistant-surgeon Bulteel, the hospital of which regiment had also been removed to Windmill Hill.

But a circumstance which occurred in the 43rd is calculated to set at rest the question of transmission by contact, as well as by inhaling the atmosphere of a ward in which Yellow Fever patients are placed:—no experiment could possibly be devised more calculated, to put these matters to the test.

Not having civil servants, from the breaking out of the epidemic till the 13th of October, (a period of exactly one month.) we had from one to three men sent us, daily, from the barracks or camp,† at the northern extremity of the rock. They were relieved every 24 hours, with one or two exceptions. The numbers which had been sent, during the month, amounted to 69. Here then we have an experiment on a scale that cannot be rejected by the most fastidious.

I give (Addenda T., p. 240) the names of those men;‡ and in order to prevent doubt respecting so important a document, I procured the signature of the Adjutant of the Regiment, which will be found affixed to it. The date of each man's tour of duty at the hospital is added.

By an examination of that return, it appears,—

1st. That two-thirds of those men were not attacked.

2nd. That while individuals in camp who had never approached the sick, but had more or less communication with the town, (mounting guard, &c.) were taken ill, in the proportion of 1 in  $3\frac{1}{4}$ , it happened that those Orderlies afterwards suffered in a considerably less proportion.

3rdly. That one-third of those attacked had, subsequent to their being on hospital employ, mounted guard, or taken other duties, in localities where other men had been frequently taken ill.

4th. That, keeping in view the proofs of the latent period in Yellow Fever, being confined to a few days, this return affords

\* Plan, Letter (F.)

† Plan. Letters (M.) and (P.)

‡ I handed in this list, with the various particulars, to the Commission appointed to examine into the Origin, &c., of the Yellow Fever; and I presume it was forwarded to the Colonial Office, with their proceedings.

the clearest evidence that the attacks, in none of the cases, can be attributed to the men having been employed on hospital duty. It will be seen that some were attacked many days beyond the estimated latent period, and some not for several weeks; all being, in the interval, exposed to the influence of local causes.

It is needless to refer to what the duties were of the above 69 men, constantly in the wards;—supporting the sick while at the night chair;—washing them;—assisting to clean their mouths when affected by hæmorrhage;—making their beds;—shifting their linen;—acting as night guards;—and in short, occupied in every office about the sick.\*

Independently of these, others had been employed in taking foul bedding to the barrack stores, without anything taking place calculated to lead to the belief of transmission by "*fomites*." Concerning those persons employed as washerwomen by the civilians, I am able to assert, after every inquiry, that the results were similar to those which occurred amongst the individuals whose names are given in the return of washerwomen for the regimental hospitals (Addenda U., p. 243); and that the attacks of those women could in no case be reasonably traced as an effect of washing the linen of people labouring under Yellow Fever.

Among the medical officers attached to the regimental hospitals in the southern district similar facts to those regarding the orderlies, &c., were observed; none having been taken ill, till an advanced period of the epidemic, when families in private houses in the neighbourhood had been long affected. This was most remarkable in an establishment formed towards the close of the epidemic on ground near the orrillon ditch,† North Front; where none,—among susceptible‡ orderlies, and medical officers, peculiarly susceptible from their having not long previously arrived from England,—were attacked with the fever.

I shall next refer to the non-transmission of the Yellow

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\* Our experience in respect to hospital attendants was confirmative of what is recorded on many occasions; especially at Barcelona, in 1821, (as stated in the Report on the Documents of Dr. Chervin, read at the Academy of Medicine of Paris,) where, in one instance of 150 persons appointed to attend on certain houses occupied by sick, the proportion taken ill did not exceed that of the general mass of inhabitants attacked. Of 32 persons employed at the hospital called Nazareth, near Barcelona, in the same year, not one was attacked during the 37 days that hospital was established,—but several suffered some time *after their return to the city*, when the establishment was broken up. Another similar instance occurred in the Queen's Hospital, near Barcelona, in the same year. See also Appendix No. III., p. 284.

† Plan, Letter (G.)

‡ This word is used by me throughout, in its common application, in reference to Yellow Fever; that is, that the individuals in question have not had a former attack.

Fever to patients under treatment in hospital for other diseases.

Previous to the "Naval Hospital" having been appropriated, *exclusively*, to cases of the epidemic fever, all diseases belonging to six regiments were for some weeks treated in that building; and during the residence of non-febrile cases in hospital (with merely the general arrangement of placing them in different wards) the same results took place;—none were seized with the fever till it had previously spread among the inhabitants of the district in which the hospital is situated. This, I believe, is in accord with what happened at Gibraltar in former epidemics, and with what is so often recorded as occurring in the West Indies and other places.

At the commencement of the epidemic the population of the districts first infected, consisting of about 4,000 persons, abandoned the town by order of the authorities, and encamped on the Neutral Ground. They took with them their bedding, furniture, &c., and this was the case with families which had had some of their members previously attacked.

Had the disease been of a contagious character, what must have been the result?—naturally, that the fever would spread in the camp, huts, &c., but this was not the case.\*

There was *no spreading* of the disease from the convalescents belonging to the army who went out to camp. But one circumstance is particularly worthy of notice. I was informed by Mr. Hugh Fraser of the Civil Hospital that, of great numbers of poor who had been treated in this establishment, one third at least rejoined their families in camp, taking with them their clothes, &c., and yet no spreading of the disease! Many of these were discharged for want of accommodation previous to their being advanced in convalescence, and some while still bleeding from the mouth.† Can proof stronger than this be required?

\* The Superintendent of Quarantine in the second edition of his book (1848), p. 34, says,—“Upon the Neutral Ground there is a constant and strong breeze or current of air sufficient to destroy the contagion of any disease, and in this way the contagion not spreading is to be accounted for.” We have here an instance of the never-failing efforts of this gentleman to lose no opportunity of turning everything to his own views of contagion; for instead of there being this *constant and strong breeze* on the Neutral Ground in the summer months, the air is often stagnant, and the heat intolerable. In my frequent visits to the camp during the epidemic, I often experienced an insupportable oppression in the tents; and my opinion is that the poison productive of Yellow Fever, instead of being generated in the bodies of the soldiers, and then dissipated by strong breezes, is not produced at all on the naked surface of this locality.

† In the Yellow Fever epidemic at Leghorn in 1804, circumstances analogous to these occurred. It is recorded that 6,000 persons left that city for Pisa after the outbreak of the disease, and that the French army, *with 180 men labouring under it*, also removed to the same place; yet under these circumstances the disease was not propagated at Pisa. See *Palloni* on the Yellow Fever in Leghorn in 1804.—Addenda (I.), p. 224.

In corroboration of the non-contagious nature of the Yellow Fever, founded upon the facts already related, the course of the disease in civil life at Gibraltar furnished the most ample details. It would be an endless task to point out instances in which, contrary to all the laws of contagion hitherto observed, two, three, or more susceptible members of a family escaped, while exercising every kind of office towards an equal number labouring under the disease. It is not my intention to attach any weight to the escape of an *individual* in a family;—this we know is an every-day occurrence in diseases reputed contagious; but in diseases of this last description, men of the highest intellect and experience have been employed in obtaining an average of the numbers who may escape; some have rated this as 1 in 26, while others estimate it as 1 in 34.

Now the instances of exemption in susceptible persons at Gibraltar, bore no relation whatever to these proportions,—for did space permit, I could quote a great number of instances, in which the majority lay quite on the side of those who escaped, though “susceptible.” The weight of such facts must, among those devoted to the examination of such subjects, be of great effect, in leading to the formation of just opinions; but such persons must perceive that where a single *individual only* has been affected, while surrounded by *many* susceptible persons, the proof of its not being transmissible, becomes so multiplied, as to approach demonstration.\*

As a conclusion to be drawn from the observations of unprejudiced people, it must be stated that the number attacked bore no proportion to those exposed to contact with persons labouring under the disease.† Coincidences, giving an appearance of contagion, may, and frequently do, present themselves, during an epidemic; but it is presumed, such circumstances, will in the present day, have little chance of influencing public opinion, however ingeniously represented. The occurrence of attacks in an inverse ratio to exposure (had contagion existed) was one of the most striking facts which occurred. By the kindness of several army medical friends, I was furnished with memoranda from the different regiments, from whence it appeared that in families, among the military, the father, mother, or child, had been taken ill in 144 instances; and that the total number

\* Among numerous instances, the case of a woman named Ackerman, of the 73d Regiment, was remarkable, inasmuch as she remained throughout her illness in a shed where there were no fewer than 18 or 20 susceptible individuals, without the disease being transmitted to any of them.

† A strong circumstance illustrative of this occurred in the family of Mr. Duguid, some of the members of which having contracted the disease, while residing in the town, were removed whilst ill to the Neutral Ground, and lodged in a confined shed. In that shed several others of the family had, as I have been informed, communication with the sick; but they were not affected with the disease. Here the safety to the unaffected was manifestly owing to their having left an infected locality.

composing those families amounted to 516, of which 372 were not attacked. In these cases the exposure, did contagion exist, was infinitely greater than among unmarried soldiers, who slept separately.

It may be observed that in families the endeavours to conceal disease, are very great, on account of the unwillingness of the parties to be separated. Having examined the returns of those attacked, out of the above 516, I can state that it was impossible to trace the illness of any one member of a family to contagion. Whenever more than one were attacked, they had been equally exposed to local causes; where they had not been so exposed, they remained unaffected, though in the closest contact with the sick relative in the house or tent. This was very clearly established in many instances.

"Ninety-two women," says Mr. Amiel, "of the 12th Regiment and 190 children, who were never allowed to re-pass Bayside barrier, have continued perfectly healthy; and one woman only (the armourer's wife), who during the period obtained leave to enter and stay a few days in the garrison, caught the fever and died of it."

"Several of these women passed the night in the same beds with their husbands, attacked with, and labouring under, the epidemic fever, and besides continued, as well as their numerous children, to use the same bedding after the men had been removed to the hospital; but in no instance was the disease contracted by the wife or the children, even after that full exposure."

No regiment had so few women or children exposed to the malaria of the Rock as the 12th; and the exemption was precisely in proportion. We seem to approach demonstration at every point upon which this question can be taken up.

Other circumstances must not be omitted, in order to show that there was no analogy between this epidemic and diseases reputed contagious. In epidemics of small-pox, measles, scarlatina, &c., other diseases exist simultaneously, but in 1828, as in similar epidemics at Gibraltar and elsewhere, all other diseases seemed to pass into its form, or to be altogether prevented from appearing.\*

In diseases reputed contagious, changes of temperature have not so manifest an influence in their disappearance; and it seems incompatible with contagion, that Yellow Fever epidemics should, as they are known to do, appear and disappear at particular seasons of the year.

In regard to the efficacy of fumigations in preventing the propagation of diseases reputed contagious, the analogy with

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\* I am to be understood as speaking of what seems to be a general law: exceptions certainly occurred.

Yellow Fever ceases. Having been very intimate with Dr. Arejula, I received from him a short memoir on the inefficacy of fumigations. He intended that this memoir should have been published with his work on Yellow Fever in 1806; but it was suppressed, by order of his then absolute Government.

Let us enquire from whom our soldiers could have been receiving the contagion during the four months the disease continued. Were our men mixing with the civil population, and entering houses which might have contained sick persons?—No;—the soldiers were for more than three months of the period encamped outside the walls, without having been allowed to enter the town except on duty. It may be said, that the mass of those attacked merely marched to their guard-houses, to which, certainly, the inhabitants were not in the habit of resorting, and where the soldiers, as certainly, came in contact with no persons suffering from Yellow Fever. Arrived at his post, a soldier was placed as sentry, not in the midst of a dense population; not with people about him, from whom disease might be transmitted; nothing of this occurred. The guard in charge of the ruins of Pompeii is not in the midst of a deeper solitude, than were often the soldiers at Gibraltar, as they inhaled the “death-blast,” *in districts abandoned by the inhabitants*. Contagion!—Our men were far removed from contact with sick people;—far out of hearing;—far out of sight. If, in the face of all this, the doctrine of the contagious property of Yellow Fever find active supporters, likely to influence Government, I know of nothing which can set the question at rest, as far as British colonies are concerned, except the intervention of a British Parliament.

LOCAL CAUSES.—Together with what may be inferred from the foregoing, many circumstances occurred demonstrative of the local origin of the Yellow Fever at Gibraltar in 1828; and similar observations have been made wherever that disease has been known to prevail.

Among the troops, those who suffered in the first instance, were the Royal Engineers, quartered in the lower part of the district,\* in which the fever first made its appearance. It was remarkable, that the 43rd next suffered seriously, though quartered at the Casemate Barracks, at the opposite (northern) extremity of the town† This admits of explanation. The 12th Regiment, in barracks in the town range‡ (situated like the Engineers in the lower part of the district first infected), left the town for camp on the Neutral Ground on the 5th of September; at which time a few cases had appeared among

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\* Plan, Letter (R.)

† Plan, Letter (P.)

‡ Plan, Letter (Q.)

them; and, as long as that regiment discontinued furnishing town guards (18 days), they ceased to have fevers, except among those who had from their duties as orderlies, clerks, &c., remained in town. The 42nd and 43rd, both in the same range of barracks, had been taking by turns for three weeks each, the duties in the neighbourhood of the sickly district; and as it then happened to be the turn of the latter the men had these guards together with duties on other posts abandoned by the 12th. With this exception the 42nd and 43rd were similarly situated.

Under these circumstances, the 43rd Regiment passed from a state of perfect health\* (not having a case of fever of any kind for a fortnight previously) to that of deplorable suffering; and before the disease had made an impression on any of the other regiments of the line the 43rd suffered a serious loss. This was at a time when the sentries were solitary beings, in the midst of desolation.

The effect of locality was here manifest.

The posts which seemed to cause the greatest number of sick, were those called North Flat Bastion, Southport, Convent, and Provost.†

Not only were the men of the 43rd among the earliest attacked, but the disease assumed, in their case, an extraordinary degree of malignancy. The late Dr. Hennen (principal medical officer) in his report to the Director-General of Hospitals in London, states that "the impress of death seems to be on the men of the 43rd, who have been attacked."

Acting Deputy-Inspector Dow also made a statement of the extraordinary virulence with which the disease appeared in the 43rd, and it was noticed by the medical gentleman who acted with me as well as by others. Circumstances like these are commonly known to accompany diseases arising from malaria; they occur frequently in Yellow Fever.‡

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\* There had been upwards of 30 cases of cholera, and one death, in the regiment after its arrival from Portugal some months previously.

† Plan, Letter (W,) Moorish Castle.

‡ Similar facts have been observed by Dr. Jackson at Philadelphia, also by Drs. Monges and Hodge of that city. It is stated by the two latter, that in one place, or at a particular time, almost all their patients recovered; while in another place, or at another time, the disease was so virulent that almost all their patients died. In the mild epidemic of 1793 at Philadelphia, M. Monges, out of 300 patients, lost only a child. Authors abound with accounts of these differences. In an epidemic at Cadiz we are told that the regiment of Saragossa lost 800 out of 1,200. By a statement of the authorities at Barcelona, the loss at one establishment in the epidemic of 1821 was 1,265 out of 1,739. M. Berthe, at the head of a commission during an epidemic at Seville, states that the loss in one part of the town was only 1 in 18, while in another it was one-half. The same gentleman has observed of the epidemic at Cadiz in 1800, that in some parts of the town the mortality was tenfold greater than in others. In an epidemic at Xeres it has carried off one-third of the population, consequently in all probability four-fifths of those attacked. M. Moreau de Jonnés states, that in an epidemic which he saw in Martinique almost all attacked died. In Sir William Pym's book we have many instances of great mortality at one point, and of comparatively little loss at others.

In the Gibraltar epidemic of 1814, Mr. Donnet, Surgeon of the Naval Department, did not lose more than 1 out of 32 cases; while the disease at other points, was much less manageable. In 1828, while it ran so mildly through a family at one point, as not to destroy a single victim, the disease carried off the whole of a large family at another; and out of 34 Jews treated in the Civil Hospital, in the early part of the epidemic 33 died. Are not these things far from characterising epidemics reputed contagious? We know that contagious diseases, will differ in force, as it respects individuals according to certain circumstances, but constant differences, such as these, are not found to take place.

The greater security which has been remarked to exist in upper floors in diseases from malaria was, in some instances, distinctly verified at Gibraltar in 1828. In a particular instance, the whole of a family, living on an upper floor, escaped; while every one of another, on the ground floor of the same building, suffered.

The existence of local causes was also demonstrated by persons remaining well on the Neutral Ground, while attending their sick relatives, but being attacked on their return to town, a considerable time after, when not in contact with sick.

The late Assistant-Surgeon Fraser, 73rd Regiment, who had been living in the lazaretto establishment, on the glacis near the Neutral Ground,\* remained well while there, but was seized, on his return within the garrison, which exposed him infinitely less to contagion, had such existed. One man having a family of several children, had a part of them attacked early in September, while residing in the quarter where the disease first appeared. He removed into camp, where the children, who had been sick, joined the family after recovery, and the whole continued well whilst they lived in camp. He returned home some time after, when he conceived he might do so with impunity, (expurgations, &c. having taken place), but the remainder of the family were soon attacked. It would be an endless undertaking to quote all similar instances which occurred. The medical world may find abundant details in a work since published by Dr. Chervin.

The inefficacy of perfect seclusion from persons labouring under the disease, as well as from all who came in contact with the sick, was clearly exemplified in 1828, and other epidemics of Gibraltar, (see the remarkable dockyard case in 1813.†)

One remarkable instance, under my own observation, occurred in the house of a lady residing near Rosia Barracks.‡ Her niece was attacked with the disease, which ended in death, though the most rigorous segregation had been adopted for some time previous. In another case, a soldier of the garrison,

\* Plan, Letter (H.)

† Addenda, (C.), p. 210.

‡ Plan, Letter (S.)

who had been in attendance on the sick in hospital for three days, became alarmed, and left his post, without having been affected. After an interval of several days, however, passed in solitary confinement in the Provost Prison (Moorish Castle),\* this man was attacked and died. I should be at no loss for many others under this head.

The admission by all (including Sir W. Pym), that whenever the Yellow Fever has been epidemic in Spain, it has appeared at a particular season, and declined, with equal regularity, so soon as a low temperature has set in, must always have great weight in establishing the local origin of the disease; heat being known to favour the production of diseases originating from noxious emanations.†

Whatever was observed as to the influence of local causes in producing the Yellow Fever in 1828, had been noticed by impartial people in the progress of former epidemics at Gibraltar.

The neighbourhood of Boyd's buildings,‡ famous in 1804, again became one of those points from whence the origin of the disease emanated, though no trace of the houses remained

\* Plan, Letter (W.)

† Some occurrences affecting the lower animals, observed during the epidemic, were proofs of the existence of a morbid atmosphere. Dr. Arejula had previously observed, that in Cadiz, Malaga, &c., birds abandoned those places while the disease continued, but returned on its disappearance. He informs us that he announced to the inhabitants of Malaga that their epidemic was about to leave them, merely because he saw the sparrows returning to their usual haunts; he tells us that dogs especially suffered from the same symptoms as the persons attacked; next were cats, then horses, fowls, and canaries, the latter died after throwing up blood; dogs and cats also suffered this last symptom, but vomiting and dark stools were more common in the latter. "Of three dogs (setters) and two cats in my house none escaped an attack, and in all I observed the black discharges from the bowels; the dogs recovered and the cats died. The horses which I saw die had either the marble coldness of the extremities, or general convulsions. It may be presumed that pigeons and other birds suffered the same disease, but I did not witness them, neither did I witness this in fishes; but it was stated that at the time there was great mortality among them." I am not aware that quite so much as this was observed at Gibraltar in 1828, but it is beyond all doubt that there was an extraordinary mortality among animals, as dogs, cats, monkeys, parrots, &c. In a small and remarkably ill-ventilated yard, in which several servants were taken ill, three dogs died. Mr. Boufante, a merchant at Gibraltar, drew out a list of eleven animals that died on his premises. Mr. Duguid, another merchant, also informed me that nine or ten dogs had died on his premises, and the skin of the greater number became yellow. Two of these dogs I myself saw, while still alive, with yellow skin. It was stated by one of the servants that the oldest of these animals had a short time previous to her attack abandoned the place where she usually slept, and endeavoured to establish herself in an upper part of the building. Mr. Danino, also lost several animals. A monkey, belonging to Mr. Griffiths, died, the skin and eyes being distinctly yellow. A goat-keeper, residing on the southern part of the Rock, lost a considerable number of his flock. I was informed by some of the private practitioners that they saw black vomit in some of the animals which perished. It is a curious fact that in the same place in which Mr. Duguid's dogs were seized in 1828, a fine healthy pointer brought from Spain died the following year, labouring under precisely the same symptoms,—yellow eyes and skin, bloody dejections, &c. In the examination of this dog no inflammation of any of the viscera was discovered.

‡ Below letter B. on plan.

which had existed in that year.\* As in former epidemics, the disease was confined to the town for some time, and was then found to break out at several points in the southern part of the Rock.† Similar to this is the progress of other diseases not arising from contagion.

If it be asked, why local causes produce the Yellow Fever at Gibraltar, &c., only at intervals of many years? I would ask, in turn, why the cholera which raged at Ceylon in 1817, had not appeared there for 48 years previously? I would ask why, in 1812, ague appeared, for the first time in certain high situations in Portugal? It might be asked, why there was an interregnum from the appearance of the Yellow Fever at Martinique, from 1807 to 1816, as stated by Dr. Dariste, who practised in the island for 30 years, and who tells us that the old inhabitants speak of intervals of 25 years between the prevalence of their epidemics. A long interregnum is well known to occur at Antigua, St. Vincent's, Sta. Lucia, Philadelphia (46 years) Charleston (41). &c., although the commerce of these places usually varies but little.

I would ask, why the very remarkable disease which appeared a few years ago in Paris, to which the name of *Acrodynia* was given by the Faculty,—had not before appeared?

A hundred such questions might be asked to which it would be equally difficult to reply, as that respecting the periodical appearance of Yellow Fever. But the question is taking its natural course, and for 100 advocates, which 30 years ago, might be found on the side of contagion, 10 will not now appear (in countries where it is permitted to express free opinions) who support that side of the question. Such may be really worthy persons who have not had sufficient opportunities of studying the disease,—and those *interested* in maintaining its exotic origin.

It must be confessed, by all unprejudiced persons, that the closest investigations have failed in the discovery of the immediate, essential, *sine quâ non*, cause of Yellow Fever.

Up to the latest authorities, ‡ we can obtain no more satisfactory hypothesis than the high probability of the influence of certain terrestrial emanations. What seems quite established

\* Dr. Prout, in his Bridgewater Treatise, expresses an opinion to the effect that a small quantity of malaria may prove a source of considerable mischief.

† In a Yellow Fever epidemic at Seville in 1819, the disease was limited to the quarter called Sta. Cruz, and certificates from physicians, then given to Dr. Chervin, prove that *out of that quarter* servants in lazaretto, hospitals, &c., were not attacked. At Arcos two large districts of the town remained uninfected, when the Yellow Fever appeared there in 1801 and 1804, though the communication with them was free.

‡ Dr. Bartlett, professor of the Transylvania University, in his splendid work "The History of the Fevers of the United States, 1847," and Dr. John Davy, Inspector-General of Hospitals, in his Report on the Yellow Fever, lately prevalent among the troops at Barbados. See Addenda (K.), p. 226. As also in his very valuable notes to the "Account of the Yellow Fever Epidemic of Guiana," by Dr. Blair, (London 1850.)

in my mind, however, is, that whatever else the special mysterious agent may be, it is never produced by *contagion*.

The days have passed when the malaria from marshes was considered as essential to the production of Yellow Fever, and now the general opinion seems to be, that this disease is developed both on high and low lands;\*—that, in fact, even on hills of considerable height, far from the vicinity of marshes, it appears occasionally as an epidemic.

Facts collected from a series of meteorological tables drawn up by engineers and others at Gibraltar, in the course of many years, are calculated to disprove the influence of certain agents, which have been considered indispensable to the production of Yellow Fever. These agents may, however, have some effect on particular occasions.

*With respect to a temperature much above the usual standard of the season at which the disease occurs*, I find that though heat has been one of the conditions under which Yellow Fever has always been produced; the Gibraltar epidemics have not occurred in the *very hottest years*.† The summer of 1828 was, on the whole, one of the coolest remembered for a long time, for, though it averaged for the *five* warm months (from June to October) rather a higher range than the preceding year, the difference is too trifling to be considered, when it is observed that several healthy years had been much hotter, and that in the *three* months in 1827, corresponding to those which *preceded* the epidemic of 1828, the average heat was greater by  $2\frac{1}{2}$  degrees.

For many days the contrast between our calamitous state in the garrison, and the beautiful scenery presented in the bay, and from the opposite coast, heightened by the clear atmosphere, and the occasional refreshing breezes from the Atlantic, was very remarkable.‡

Indeed, the very flourishing state of a flower-garden, near the scene of desolation at our hospital, was far from conveying pleasurable sensations.

*State of Barometer.*—On this point there is little to be said. By observations for 10 years, from 1816, its greatest height had been 30·90 inches, its lowest descent 28·62 inches.

*Regarding the quantity of rain*, to the influence of which great weight has been attached, I have in my possession a table of the indications of the pluviometer at Gibraltar from 1790, showing that the Yellow Fever has prevailed both in seasons

\* See, especially, Macculloch on Malaria. &c.

† Similar facts have been noticed by Humboldt, in treating of the origin of Yellow Fever at Vera Cruz (Political Essays, vol. 4, p. 202.) We have also statements of the best authorities as to the epidemics in Spain not appearing oftener in the hottest seasons, though one or more such instances may have happened. In 1752 the heat was so excessive at Gibraltar, that the inhabitants during the night fancied their houses were on fire, and birds forsook their nests, but no epidemic took place.

‡ I think Miss Pardoe, in her "City of the Sultan," speaks of some such contrast during the prevalence of the plague at Constantinople.

remarkable for the fall of much rain, and in those in which comparatively little has fallen.\*

*With respect to particular winds*, I may observe that (contrary to what appears to have been the case in 1804) there had been no unusual prevalence of easterly winds, either preceding or during the progress of the epidemic. It appears that, in the three months ending August, 1828, there were only 39 days of easterly wind, and only 19 in the months of July and August; whilst in the corresponding three months of 1827 (healthy year), there were 59 days of easterly wind; 39 being during July and August.

I think it must appear, that the examination of meteorological variations, furnishes no satisfactory information respecting the cause of Yellow Fever at Gibraltar.

It may be as difficult to explain the deviations of nature from her ordinary course, as it is to account for her more usual operations. We can only say, that such things are so ordained by the will of God. Admitting the production of Yellow Fever, in particular localities, from concurrences in the external agents, is but adding one more disease to the numerous class already withdrawn from the ranks of contagion by our reason and experience. Who now seeks for the cause of spasmodic cholera, epidemic dysentery, influenza, &c., otherwise than in an assemblage of circumstances, in each case peculiar, and possessing a power of excitement specific in its nature? †

It seems to be assumed that, during Yellow Fever epidemics, chemical tests have discovered no very appreciable alteration in the air; this may be the case, and yet nobody can doubt that the air inhaled in those places may be most deleterious to health. But during what epidemics have chemists, competent to such a task, been employed on those investigations? No where, I may venture to say, in such a manner as to produce conviction on this point. Is it likely that peculiar chemical properties could be detected in the atmosphere of all places where remittents, intermittents, &c., reign? I presume this could not be expected; but experiments hitherto attempted for the purpose of discovering the precise chemical composition of the atmosphere, during a Yellow Fever epidemic, cannot be considered as otherwise than vague and undecisive.

However contagionists may dwell on the exotic origin of the Yellow Fever, it can be always discovered that they have

\* In the healthy season of 1796, 62·87 inches of rain fell; in 1806, also healthy, 14·76 inches only; in 1828, 21·50 inches;—the average being about 30 inches.

† As our experience increases we feel the force of the opinion of Leibnitz, that the diseases to which the human frame is subject are as various as the colours of flowers.

In Humboldt's "New Spain" we have the following passage:—

"The phenomena of life are, no doubt, subject to immutable laws; but we know so little of the whole of the conditions under which disease is introduced into the functions of the organs, that the pathological phenomena appear to exhibit to us, in their succession, the strangest irregularities."

a secret feeling as to the development of this disease being dependant upon some assemblage of circumstances connected with season, hitherto not fully understood, and that many of them are found ready to exclaim with Arejula,\*—

“*In a word, while the days lengthen, and the sun approaches our hemisphere, we may be sure that the fever, which has so afflicted us of late years, will not attack us; but when that luminary begins to retire from us, and during the whole period of its retiring, we may fear it; especially if we have had a sterile season, and if hot and dry winds have prevailed for many days successively.*”

Many of the facts which I have advanced concerning the last Yellow Fever epidemic at Gibraltar will be found corroborated in the writings of the following gentlemen who were present, viz.—Dr. Thomas Smith, then surgeon to the 23rd Regiment;† Mr. R. Amiel,‡ then surgeon to the 12th Regiment, who had seen three epidemics previously at Gibraltar, Mr. Hugh Fraser, for some years surgeon to the Civil Hospital, having formerly served in the 12th Regiment;§ Mr. P. Wilson,|| assistant-surgeon, attached for many years to the Civil Hospital, having been previously in the naval service.

In Dr. Smith's paper in particular (reviewed in the United Service Journal for February, 1831), the public have an opportunity of seeing how cruelly the statements of the Superintendent of British Quarantine, to the Spanish Academy, were calculated to operate against the deepest interests of Gibraltar, by favouring the necessity, in epidemic seasons, for our neighbours cutting off free communication with that garrison, which, notwithstanding their usual very kindly feelings, they are too prone to do. See also Addenda (G.), p. 216.

From the writings just referred to, it must be evident that I have not been the only person, or the first, to take the liberty of impugning the statements of Sir William Pym\*\* on subjects connected with the Yellow Fever at Gibraltar.

\* Arejula on Yellow Fever, p. 228.

† “*Brief Sketch of the Fever which prevailed at Gibraltar in the Autumn of 1828; together with Observations on the Answers of Sir William Pym to Queries from the Royal Medico Chirurgical Society of Cadiz, addressed to him on the origin and nature of that disease.*”—(Edinburgh Medical and Surgical Journal, No. 106.)

‡ Edinburgh Medical and Surgical Journal, April 1831.

§ London Medical and Physical Journal, January, March, April, and May, 1831.

|| Nos. 352, 353, and 354 of the Lancet, afterwards translated into French at Paris, and notes added by Dr. Chervin in Dec. 1830.

\*\* In a letter, dated 23rd April, 1847, addressed by Sir William Pym to the Lords of the Council through the Hon. W. Bathurst, relative to a Report on the Fever at Boa Vista, by Dr. McWilliam, is furnished a copy of an official Report from the Board of Physicians, &c., convened at Gibraltar, by an order of the 24th January, 1829, for investigating the question as to the liability or non-liability to a second attack of Yellow Fever, to which the signatures of the several members are affixed, with the names of Dr. CHERVIN and Mr. FRASER—*dissenting*.

In the second edition of Sir W. Pym's book, however, p. 279, where a copy of the same document is professed to be given to the public, we find a list of the members at the commencement of the Report, but the signature attached is merely “*LOUIS, President,*”—*whereby the world is led to believe that the Board were unanimous.*

# REFERENCES TO THE PLAN OF GIBRALTAR, IN ILLUSTRATION OF THE YELLOW FEVER EPIDEMIC OF 1828.

A.—Nos. 24 and 25 districts, in the southern and elevated part of the town, 80 or 90 feet above the level of the sea. For the most part rocky, drains and cesspools consequently very near the surface. Fig. 9 considered as the point where the first cases of the epidemic appeared.

B.—Civil Hospital, a well-ventilated establishment, with spacious wards, about 90 feet above the level of the sea; in charge, during the epidemic, of Surgeon Hugh Fraser, and Mr. Peter Wilson, Assistant Surgeon.

C.—New Protestant Church (now a Cathedral) a little to the south of King's Bastion, used as an auxiliary hospital a short time previous to the termination of the epidemic.

D.—Naval Hospital, on a rocky site, about 80 feet above the level of the sea, has accommodation for the sick of six regiments. The wards are spacious, and admit of communication throughout; there is, besides, a wide corridor with entrances to the wards. This hospital, however, being several hundred feet lower than the rock behind it, the medical officers and hospital servants did not experience total immunity from attacks of Yellow Fever, as did those serving at the barrack, used as a temporary hospital, on Windmill-hill.

E.—Ordnance Hospital, on a rocky site, at a great height above "Naval Hospital."

F.—Windmill-hill Barrack, usually occupied by one regiment; used as an hospital by the 73rd Regiment, from the 25th September to the end of the epidemic; situated on a rocky site, about 400 feet above the level of the sea, and having perfect ventilation on its south, east, and west sides. On the north side there is but a narrow passage between it and a stupendous mass of rock.

G.—"North Sheds" Hospital, three wooden buildings, consisting of ground floors only; erected, towards the close of the epidemic, on a low piece of ground close to "Inundation," outside the "North Front" defences. Here, where ventilation was perfect, as at Windmill-hill, no medical officer or hospital attendant\* was attacked with Yellow Fever.

H.—Large marquees pitched on the *glacis*, outside the North Front defences; used as a lazaretto, for the reception of the civil poor, in the early part of the epidemic; in charge of Assistant-Surgeon Fraser, 73rd Regiment.

I.—Sheds established about the end of December, as an hospital of observation, and for the non-epidemic cases of three regiments.

K.—Tent Hospitals for non-epidemic cases among the civilians; this the extreme point of our lines towards Spain.

L.—Civil encampment at north front, to which between 4000 and 5000 of the inhabitants retired during a period of three or four months. This locality had been found a place of refuge in epidemics of former years. Ferdinand VII., King of Spain, contributed a quantity of wheat to the poor on this occasion. The tents were supplied by the British Government. It was agreed by all parties, after investigation, that the expected immunity was experienced here, although great numbers of convalescents, friends, and relatives, from within the walls of the town, had freely mixed with the people in these tents. The soil is sandy, with the exception of a few acres cultivated as gardens, &c. The length of the isthmus from the British to the Spanish lines is 1650 yards; its breadth, from Gibraltar Bay to the Mediterranean, varying from 1200 to 1750 yards.

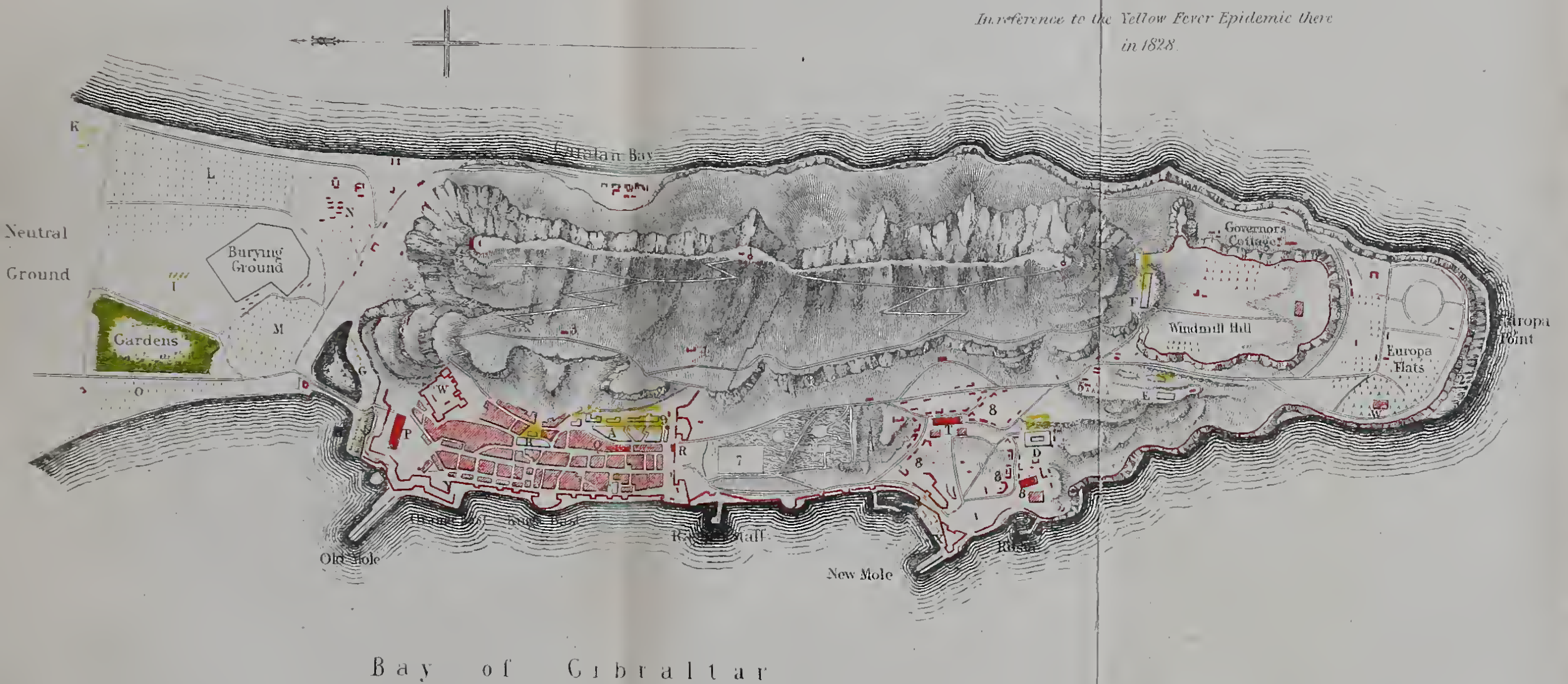
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\* The following were on duty at this hospital:—Staff-Surgeon Barry, Surgeon McLeod, 42nd Regiment; Assistant-Surgeon Galeani, 43rd Regiment; Assistant-Surgeon Davis, attached to 43rd Regiment; Assistant-Surgeons Gillice and Dick, 12th Regiment; Assistant Staff-Surgeons Bell, Fagg, and Moore; Hospital Servants,—Thomas Ormsby, Samuel Cordingley, William Blevin, and Joseph Davis, 12th Regiment; John Watt, 42nd Regiment. With the exception of Mr. Gillice, all the medical officers had but lately arrived from England, and neither officer nor man had had Yellow Fever previously.

PLAN OF THE  
ROCK, TOWN AND TERRITORY,  
OF GIBRALTAR.

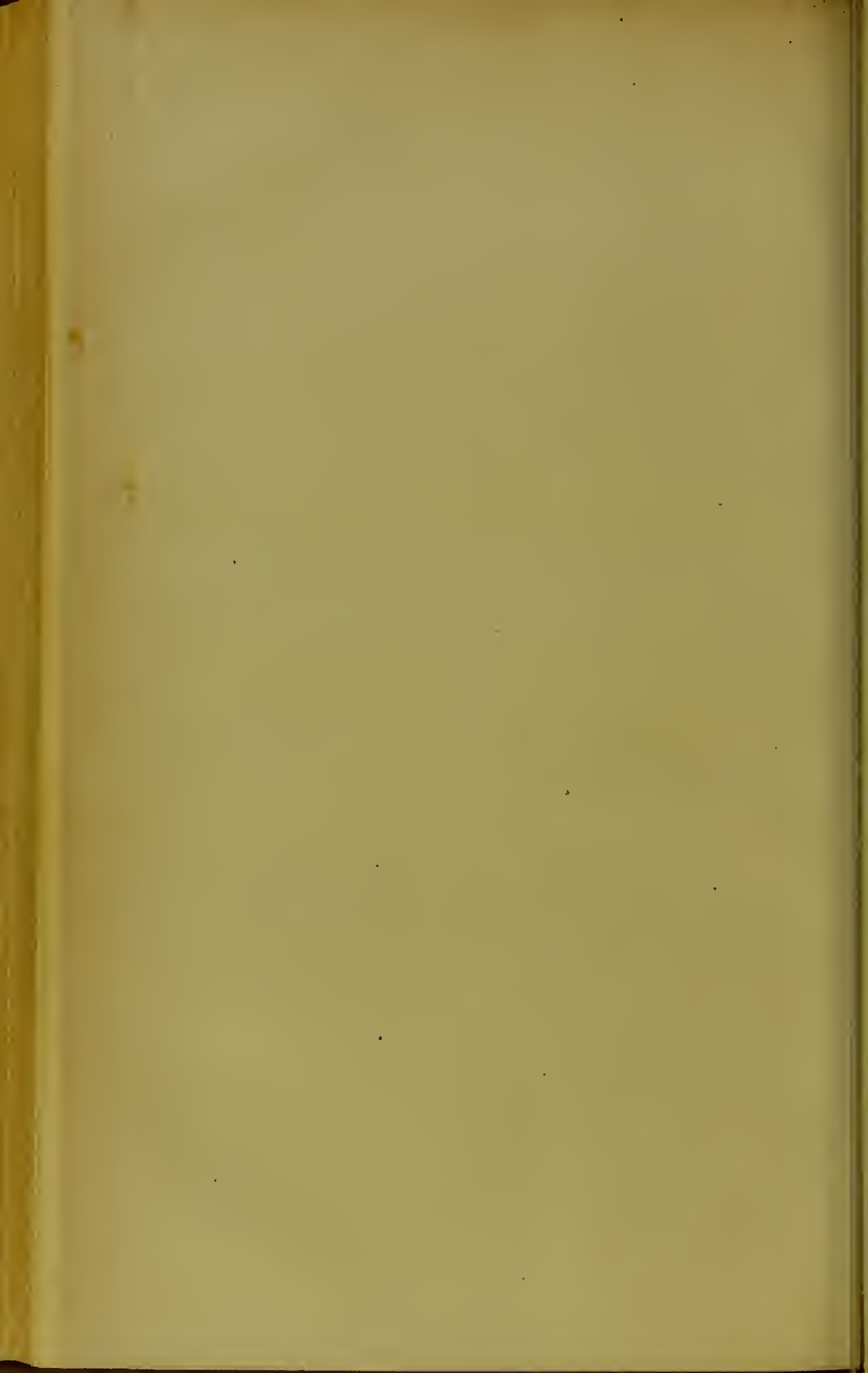
Mediterranean Sea

In reference to the Yellow Fever Epidemic there  
in 1828.



Bay of Gibraltar

500 300 0 1 2 3 4 500 Feet



M.—Military encampment, occupied by the 12th Regiment, (which got under canvass on the 5th September); the 42nd Regiment (which got under canvass on the 22nd September); and the 43rd Regiment (which got under canvass on the 17th September). This camp was established about 12 days after the first appearance of Yellow Fever in the town. The general exemption from attacks by the soldiers limited to this camp has been denied, but that such exemption really existed is shown in the Report.

N.—At this side of the isthmus, many of the respectable classes, with some of the Sappers and Miners, took refuge, and obtained the usual immunity.

O.—A village of wooden houses, then occupied during the warm months by families in comfortable circumstances as being cooler than the town, and considered as a proper place of refuge. This village has been since destroyed.

P.—Casemated Barrack, near North Barrier, within the town; occupied on the breaking out of the epidemic, by the 42nd and 43rd Regiments, which afterwards encamped with the 12th Regiment at North Front.

Q.—“Town Range” Barracks, occupied by the 12th Regiment, previous to their encampment.

R.—“Hargrave’s Parade” Barracks, immediately within the southern part of the town; occupied at the commencement of the epidemic by the Royal Sappers and Miners.

S.—“Rosia” Barracks, in the southern district, close to the little bay called Rosia; situation not elevated. Occupied by the 23rd Regiment until the 14th September, when they encamped on “Europa Flats,” which have a rocky surface, and were previously considered as a place of refuge. These Flats are about 100 feet above the level of the sea; and when encamped on them, the 23rd experienced immunity from the disease.

T.—“South” Barrack, built on a commanding and rocky situation, about 80 feet above the sea. Rooms large and well ventilated, occupied by the 94th Regiment, until the 10th of October, when they encamped on Windmill-hill.

W.—Old Moorish Castle, at a considerable height within the northern extremity of the town; where some companies of the Royal Artillery were quartered until the end of September, when they encamped partly above the “Sand Pits,” marked 6, and partly on Windmill-hill.

X.—Sheds in dockyard, converted, as well as a brig in the New Mole, into convalescent hospitals, towards the close of the epidemic.

Catalan Bay, partly occupied by fishermen, and partly as a military post, to which there is an approach for foot passengers by a narrow way. The immunity experienced at the open camp-ground was not enjoyed at this village, which is on a sandy beach, backed by 1400 feet of rock, and therefore without free ventilation.

#### *Reference to Figures on Plan.*

Fig. 1.—Signal-house, 1276 feet above the level of the sea.

Fig. 2.—Lower Europa, or Europa Point, 60 feet above the sea; where, according to Sir William Pym, the late Colonel Fyers placed himself in quarantine during the epidemic of 1804,—whereas the garrison orders of Gibraltar show that officer performed his duties in garrison with his company, whilst residing on this Point at that period. The locality, however, proved then a place of safety, as it subsequently did in 1828.

Fig. 3.—Bruce’s Farm, 712 feet above the sea.

Fig. 4.—Ince’s Farm, 680 feet above the sea.

Figs. 5, 5.—Two Camps, “Poco Roco” and “Buena Vista,” situated high up the rock, near Bruce’s Farm, and to which the Sappers and Miners were sent in the early part of the epidemic, in the belief that it might be found a safety spot. The expected immunity not having been found there, the men moved to the Neutral Ground.

Fig. 6.—Camp ground, near “Sand Pits,” not far from the New Mole, occupied at the commencement of the epidemic, by one company of the Royal Artillery.

Fig. 7.—Parade-ground, in the midst of an extensive space, laid out in public walks, and ornamented grounds for the recreation of the inhabitants.

Fig. 8, 8.—Dwellings in the district called “South,” in which several cases of Yellow Fever occurred *previous* to the attendants on patients in the “Naval Hospital,” (D) being attacked.

## WEST INDIES.

Many unprejudiced persons object to drawing conclusions as to non-contagion from the experience of one place only, and I hope I may, therefore, be excused in stating, from some of the highest authorities, the results, at many places, where the disease is known to have appeared.

Those who have entered into the subject of Yellow Fever, must be aware of the impossibility of giving, even a slight sketch of all that has been brought forward on this question. The discussions, up to a certain period, may be said to be condensed in the works of Blane, Fellowes, and Pym, on the side of contagion; and of Bancroft, Jackson, Maclean, and Burnett on the opposite side. The elaborate works of Dr. Bancroft especially, (*Essay on Yellow Fever; Sequel to an Essay on Yellow Fever,*) embraced whatever could at the time be deemed the most essential points for consideration.

It is usual to refer to Père Labat's statement of the alleged importation of the disease into Martinique, in 1682, by the ship "Oriflamme," from Siam; to which it is objected that he has merely given vague reports of circumstances which occurred several years before his arrival in that island; and that if the disease had been contracted at Brazil, where the ship touched, it was palpably erroneous to say that it had been imported from Siam. In the second volume, page 119, of Dr. Chisholm's work on the Fevers of the West Indies, he gives an account of an importation of the Yellow Fever into the island of Martinique, in 1796, which is proved to be erroneous, in a paper in the eighth volume of the *Medico-Chirurgical Transactions*, by Dr. Fergusson, Inspector-General of Hospitals, who served with the troops on board the ship alleged to have imported the disease. There can be no doubt that among those who supported the views of Dr. Chisholm on contagion, respectable names are to be found; but even so soon after the periods to which he refers, as 1801, when it fell to my lot to witness devastating epidemics in Martinique and Dominique, my experienced medical chief, Dr. Theodore Gordon, sen., did not think it expedient to suggest any measures applicable to contagious diseases, nor did an apprehension of contagion ever escape the lips of any of my seniors.

Within the last 30 years much valuable information upon Yellow Fever has been furnished by Dr. Fergusson; and it is to be regretted that want of space precludes the possibility of extracting, as freely as would be desirable, from documents by a gentleman of such great experience, tact, and candour. His paper, above referred to, is particularly interesting, and relates chiefly to transactions which occurred while he was

principal medical officer in the West Indies, in 1816, &c. Dr. Fergusson's convictions are quite opposed to the doctrine of contagion; and it will be admitted, that the facts which he has adduced in that paper are calculated to make a powerful impression.\* He shows that, without restraint as to intercourse, situation alone (as in the present day constantly occurs among our troops in the West Indies) gives great comparative exemption from the disease,—that it “is confined, in all the islands, to the sea-coast;” and that, “at Barbados, our hospitals, of late years, have been in a regular course of importation of the Yellow Fever from the navy; but not even inoculation has been able to produce the disease upon any member of the hospital corps, by whom I may truly say that the sick have been received with open arms; for the anti-social doctrines of ideal contagions are not preached among us here, to the prejudice of duty and humanity.”

Dr. Fergusson's remarks corroborate the fact mentioned by other authors, as to “different parts of the same town being differently affected; and so limited often is their influence, that one story of a house,† or one section of a ship, will be strongly affected by it, while other parts of the same tenements remain healthy.” In the paper from which these extracts are taken, will be found details of the highest value, relative to this disease among the crew of the “Regalia” transport, employed in carrying black recruits from the coast of Guinea to the West Indies in 1816; from which it appears that the crew were in good health previous to taking in many tons of green wood at Sierra Leone,‡

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\* The results of Dr. Fergusson's experience have been officially lodged in the Army Medical Department in London, and also published since his death by his son. (Longman, 1846.) See Addenda, (L.), p. 229.

† At Gibraltar, during the epidemic of 1828, this was observed to have been the case in a very remarkable manner; and Dr. Ramsay, surgeon to the forces, states in an official report, dated Barbados, 20th December, 1825, that “in certain barracks and hospitals the very diagonal of particular apartments will afford a tolerably accurate demarcation of safe and unsafe position of beds.” See on this point also Dr. Wilson's work on Yellow Fever (1827), in which the disease is shown to have been confined to men whose berths were on a particular side, or in a particular part of a ship.

‡ It may be worth while drawing attention to an extract of a letter from Mr. Showers, ten years colonial surgeon of Sierra Leone, the first being the year (1816) in which the *Regalia* sailed from that place:—“During my ten years' stay at Sierra Leone I never saw any other fever (the ordinary fever of the country); but when a fever broke out there similar to the Yellow Fever of the West Indies, attended with black vomit, which was supposed to have been brought there from the Mediterranean by a ship called the *Caroline*, this I recognised as a different fever from the one I have just described as the common fever of the country; and to my knowledge none of the medical men then at Sierra Leone had any difficulty in distinguishing it as a new and different disease.”—(Aiton's *Dissertations on Malaria*, &c., 1832.) Mr. Showers adds, respecting the fever of 1823, that he had “his doubts whether it was imported or contagious; I am much of opinion that it proceeded from the atmosphere;” which doubts he was the more justified in entertaining from the fact that for two years previous no yellow fever epidemic existed at any port in the Mediterranean. To those who had been led to believe that the black-vomit fever had been not unfrequently exported from the coast of West

—that great sickness (chiefly dysentery) prevailed among the blacks during the voyage; and that several deaths took place; but the Yellow Fever was altogether confined to the crew; and, in the words of Dr. Fergusson, “the ship, on her arrival at Barbados, was not put under restraint or quarantine, but communicated freely with the sea-ports of Barbados, the Saints, Antigua, and Guadaloupe; landing the severally ill or dying subjects of that disease amongst the inhabitants, and at the hospitals at Barbados and Antigua, without communicating infection at any of these places; and, finally, after having undergone a thorough purification, sailing from Guadaloupe for Europe, crowded to a very great degree with rebel French prisoners and their families from the gaols, under the most dangerous circumstances of health, with a case of Yellow Fever actually dying on board the day before she left Basseterre Roads, but without communicating any such fever to the unfortunate passengers,—leaving any behind her at Guadaloupe,—or importing any at the ports she ultimately reached.” Dr. Fergusson, when speaking of an epidemic which took place in the following year, says, “What a different interpretation the facts I have collected would have borne, had the present epidemic that afflicts the islands broke out in the ordinary course of the seasons, a year earlier, at the time the ‘Regalia’ was here.” I shall only offer one more extract. “At Martinique they established a strict quarantine, particularly directed against Guadaloupe, and they have been consumed with Yellow Fevers; but at Dominique, Tobago, St. Vincents, &c., where they established none at all, they have not had, in as far as I have learnt, a single case, although at the last-mentioned islands both the ‘Tigris’ and ‘Childers’ ships of war, imported distinct, well-marked instances of the disease from *Point au Pitre*, on the evacuation of Guadaloupe.”

Inspector-General Tegart, for some years Medical Chief in the West India islands, in his official report to the Army Medical Board, March 10, 1823, says, “In the various annual reports of the medical officers in this command, I have not seen one favouring, or supporting the theory of contagion: they are all on the other side.”\* This seems the more remarkable, as isolation of Yellow Fever cases, was a measure approved of a short time before by Mr. Inspector Green, one of those gentle-

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Africa, its reputed birth-place, this visitation as a perfect stranger, and its alleged importation from Europe, must appear somewhat strange. The healthy state of the *Regalia* transport previous to her sailing from Sierra Leone, together with what appears by Mr. Showers’ letter as to the non-existence of the yellow fever there in 1816, would seem to favour Dr. Fergusson’s idea of its having been produced by the green wood just laid in previous to her sailing, and to “foul ballasting that had not been changed for years.” In an official report of Mr. Hartle, it is stated, on the authority of Mr. Mortimer, of the Royal Navy, that the *Nayden* frigate having taken in green wood at Dominica lost one-third of her crew by fever.

\* Addenda, (M.), p. 230.

men who had previously served in the West Indies, about the time of Dr. Chisholm. Mr. Teggart, referring to certain cases which occurred at Antigua, in 1801, says, "The result is that this was decidedly Yellow Fever, and that the disease ceased on removal from the place, and was confined solely to those persons who occupied the room. Here is cause and effect."

The official reports of Dr. Bone to the Army Medical Department are worthy of particular consideration, he having been long resident in the West Indies. The substance of those reports are to be found in the thesis published by his son, (now of the Medical Department in the army,) in 1846.

I am not aware that any one has been more competent to speak of West Indian fever than Dr. Bone; he says in one of his reports that "the first important result which I have proved in the 'Naval Hospital'\* is that the Yellow Fever, as it is called, cannot by any possibility be communicated from one person to another." He further states, "that 35 white servants had been employed at the hospital without being attacked," and concludes by observing, "so few in the West Indies believe the doctrine, (contagion) that they may very safely be permitted to enjoy their own opinions—they cannot do much harm."

The facts brought forward by Dr. Bone against contagion, are so numerous, that I am precluded by their bulk from affording them adequate space.

Dr. Bone's son informs us that Inspector Green, principal medical officer in the West Indies in 1818, 1819, and 1820, (having the sanction of the authorities in England,) attempted to introduce the quarantine regulations then used at Malta, into the West India islands; but these measures being strongly opposed by Dr. Bone, and the great majority of the medical officers on the station, the doctrine of contagion "died a natural death" soon after the departure of Mr. Green from the West Indies.†

\* Barbados.

† The following remarks are by Dr. John Hunter—"Observations on the Diseases of the Army in Jamaica, 1796:"—

"In military physic the great improvements to be made are not so much in the cure as in the prevention of diseases, which depends altogether upon a knowledge of their causes.

"If disease arise from the air, contaminated by the foul ground of a camp, or the exhalations of a marsh, it can only be avoided by a change of situation, or by taking care not to come within the sphere of activity of such noxious causes.

"Let it be supposed for a moment possible that a mistake should be made, and that the camp, or remittent fever, be not considered as proceeding from their proper causes, but believed to depend upon contagion. It is evident that complete destruction to all must be the consequence of such an error; and, in medical history, there is reason to fear more examples than one of this might be produced.

"By a contagious disease, is to be understood a malady arising from a poison generated in the body of the sick, which produces in others a similar disease. It is peculiarly characteristic of such diseases that persons of all ranks and descriptions

So powerful were Dr. Bone's convictions before his departure from the West Indies, that, with a view to impress the truth on the minds of all those concerned, he sent a formula for the production of Yellow Fever, to the Army Medical Board,\* of which the following is an abstract,—

“Take of soldiers lately arrived in the West Indies † any number; place them in barraeks in a low, wet situation, or in the mouth of a gully, or on the brink of a dry river, or on the summit of a mountain, and to leeward of a swamp, or of uncleared ground, and where there is no water, or only bad water; let their barraeks be built of boards, or of lath and plaster, &c.

[Here follow a variety of other circumstances.]

Let these directions be attended to in Trinidad, or even in Barbados, and especially when the air is stagnant, or charged with noxious vapours subsequent to long drought, the soldiers will soon die, some of them yellow, some of them with black vomit.”

In Dr. Blair's late work on Yellow Fever edited by Dr. Davy, 1850, p. 55, we also find a formula for giving that disease to nurses:—“The way to give a Yellow Fever nurse the Yellow Fever was not by bringing him in close contact with the sick, but by discharging him or her from the hospital. After knocking about town for a few weeks and getting into the malarial districts, they would, it is likely, be brought to hospital as yellow fever patients. Several nurses discharged for bad conduct suffered in that way.”

I cannot pass over the official statements of Mr. Hartle, Deputy Inspector-General of Hospitals, who served in the West Indies, during a period of more than 30 consecutive years. His report for 1822 contains particulars of a most interesting kind, relative to the introduction of many cases of the Yellow Fever into the island of Antigua: in one place he remarks, “It is a pleasing reflection, and a source of great gratification to me,

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are equally affected by them. Wherever they prevail the old inhabitants of a country suffer as much as those that have arrived lately. But this is never the case in the Yellow Fever, remittent fever, or even intermittent fever, for such as are seasoned to the country or climate suffer infinitely less than new comers.

“But what may be considered as an *experimentum crucis*, to prove the non-existence of contagion is, when the sick leave their usual residence, and go to other situations which are healthy, without spreading the disease. This constantly happens in the remittent fevers of the West Indies; for the good effects of changing the air of the towns for that of the mountains is so well known that it is very generally practised; but, certainly, without the slightest suspicion of any mischief arising from any contagion carried by the sick.

“When disease arises from a cause generally diffused, separation from the sick does not avail. Thus, ships of war have gone into a harbour in the West Indies, and have had no intercourse with those on shore, or with the crews of other ships, and yet in a few days the men have been seized with the prevailing fever in great numbers.

“If these observations be applied to remittent or Yellow Fever there will be no ground for believing either to proceed from contagion.”

\* Report of the epidemic in Trinidad, 1818. † See Appendix, No. IV., p. 381.

that, notwithstanding 107 cases of Yellow Fever, as distressing and malignant as any I have before witnessed, have been, by three vessels, imported into this island since September, 1821, we have not a *single instance* of any individual but those directly exposed to the local causes [ships] having been attacked." He states that the sick received on shore from one of those ships ("Dasher" transport) were attended by Europeans. Mr. Hartle's account of the Yellow Fever on board the "*Pyramus*" frigate, which arrived in English Harbour from Barbados, on the 3rd of January, 1822, is highly important. The following are the principal facts recorded by this gentleman. Neither the officers nor men had been exposed to solar influence, or other exciting causes. One of the principal reasons assigned for the breaking out of the disease was, that this ship had been "injected with coal-tar, which, with bilge-water, caused remarkable effluvia." The only ships on the station injected with coal-tar were the above, the "*Esk*" sloop-of-war, and "*Dasher*" transport, "all of which suffered, the former and latter especially, with a similar type of disease, Yellow Fever, in its most malignant form." He states that the crew of the "*Pyramus*" were landed at English harbour, and the ship dismantled. When the limber-boards were removed, the effluvia from the hold surpassed everything which he had "ever before experienced." A boatswain looking into the hold from the lower deck, while an inspection by proper officers was going on, fainted, and passed afterwards through a formidable attack of the disease. Mr. Hartle himself, (I believe a native of the West Indies) who was one of the officers appointed to examine into the state of the ship, escaped with slight indisposition. He states, respecting the others, that "every individual present at the opening of the holds and limber-boards was attacked by the prevailing disease." Although the frigate had been only six months from England, and was believed to have been a short time out of dock, large mud-boats of filth were removed from her at Antigua, which was nine inches deep in the hold. The negroes employed in removing this mass were obliged to go on deck occasionally, so insufferable was the stench, and three of them had the disease. The after-magazine, immediately under the gun-room, was found in the worst state, and this accounted, in the opinion of Mr. Hartle, for every officer's servant, and servant of the gun-room mess having suffered. Objections having been made to the removal of the crew beyond the doekyard, after their landing on the 15th, several cases occurred up to the 30th, in consequence of the men having gone on board clandestinely; the crew were therefore encamped at some distance from the doekyard at Berkeley Fort, while a cleansing and thorough purification

took place; and, on returning on board, their health continued good.

#### AMERICA.

Passing now to the American continent, my limits admit but of a few brief remarks. Up to the year 1793, almost all the medical men in the United States were believers in the communicable nature of Yellow Fever; but each epidemic diminished the numbers, so that, in 1825, according to an American commercial almanac, while 567 were against the doctrine of contagion, 28 only remained in favour of it, throughout the whole country. The public manner in which the celebrated Dr. Rush, once a believer in contagion, retracted his opinion, is matter of historical notoriety.\* At New York the doctrine of contagion was long and ardently supported by Professor Hossack and Dr. Townsend, both of whom wrote much upon the disease. The facts which presented themselves to Dr. Beck in the course of the epidemic at New York (1822), caused his public retraction of faith as to contagion in the following year;† and Dr. Townsend appears to have admitted‡ that, of about 200 persons of all grades of the profession three or four only believed in the transmissible nature of Yellow Fever. "In 1793 the profession were almost unanimous in the belief of its contagious character, and no little courage was required to brave the storm an opposite opinion would have awakened. In this generation an equal unanimity prevails in the profession as to the non-contagious nature of the disease; and he who advances the opposite doctrine seriously, is deemed no more worthy of notice, much less a refutation, than would be an advocate at this time of the Ptolemaic system.§" The following statement from the pen of M. La Roche, French consul at Philadelphia, is extracted from a letter to a friend, 20th July, 1830. "A friend of mine, Dr. Morrel, has lately arrived from the Havannah. During a few days' passage three persons died of Yellow Fever on board; and a fourth, taken ill on board, died in the New York quarantine establishment. The sick *were all cabin passengers*, and received the germs of the fever in the port. The other passengers, who merely em-

\* Dr. Rush, [quoted by Sir W. Pym, 1st Edit., p. 208] says "He begs forgiveness of the friends of science and humanity, if the publication of that opinion has had any influence in increasing the misery and mortality attendant upon that disease. Indeed, such is the pain he feels, in recollecting that he ever entertained or propagated it, that it will long, and perhaps, always deprive him of the pleasure he might otherwise have derived, from a review of his attempts to fulfil the public duties of his situation."

† New York Med. and Ph. Journ., No. viii., p. 472. See Addenda (S.), p. 238.

‡ Chervin.—De l'Opinion des Médecins Américains, p. 11.

§ See Amer. Journ. of Medical Sciences, August 1829, p. 523.

barked at the moment of departure, without having waited in port, remained well, and that notwithstanding the inevitable contact arising from 12 or 15 persons sleeping in a small cabin. Dr. Morrel and the other passengers were placed in quarantine, but during the time every body went to see them."

Much interesting matter relative to the Yellow Fever epidemics which have prevailed in America, may be obtained by consulting various pamphlets published by Dr. Chervin.

That in North America the disease has not been propagated by the removal of persons labouring under it, even when carrying with them their bedding, &c., has been shown by observations made by medical men of upwards of 30 cities and towns, according to a report upon Dr. Chervin's documents, read at the Academy. By those documents it appears that attendants of all classes on Yellow Fever patients constantly remained exempt from the disease, *where the hospitals were placed out of particular local influences*. This it appears was the case at the hospital at Bush Hill, near Philadelphia; in that of Belle-vue, near Fort Stephens; in that of the Navy at New York; also in those of Norfolk, Baltimore, Providence, Newport, Boston, and New London. These most important facts are verified by Drs. Chapman, Redmond, Coxe, Mease, Lehman, Mitchell, Parish, Jackson, Perkin, Miller, Tueker, Thomas, Baeke, Harlon, Coates, &c., of Philadelphia; by Drs. Anderson, Brown, Walker, Drake, and Osborne, of New York; by Dr. Areher, of Norfolk; by Dr. McCauley, of Baltimore; by Dr. Weaton, of Providence; by Drs. Turner and Waring, of Newport; by Dr. Townsend, of Boston; and Dr. Lee, of New London. Proofs to the same effect, collected in the West Indies, were laid before the Academy, by Dr. Chervin, in 1827.

Dr. Pariset has admitted that the Yellow Fever "is not contagious in America,—whether it had ever been so, or has ceased to possess that property."\*

With respect to America, this subject has perhaps been more fully entered upon by Humboldt than by any other person. In his Political Essay, (vol. iv.) he mentions that at Vera Cruz the idea of the importation of the disease from the Havannah and other places had been entertained; but, by the facts which he furnishes, there seems to be no cause for doubt as to the disease being indigenous. The subject of contagion is investigated by this celebrated man in the spirit of philosophy for which he is so remarkable: we are shown to what an

\* "N'est point contagieuse en Amérique, soit qu'elle ne l'ait jamais été, soit qu'elle ait cessé de l'être."—Bulletin des Sciences Méd., tom. xii. p. 126.

Dr. P. F. Thomas, after an experience of 30 years at New Orleans, fully agrees as to this disease never exhibiting, under any circumstances, a contagious property. —See his Treatise on Yellow Fever, Paris, 1848.

extent one test of the communicable nature of a disease—that of taking people, whilst ill, into healthy districts—has been applied, and the result proved to be entirely against the doctrine; that not only at Xalapa, and higher up in the interior, but at the farm of Eneero, a short distance from Vera Cruz, the disease is found to confine itself to the persons of those who may arrive with it in their systems, notwithstanding the freest intercourse with others.\* Every observation made by Humboldt relative to Yellow Fever, is of high interest: one seems peculiarly deserving of attention; which is, that although the disease usually prevails among the newly arrived *every year* at Vera Cruz, it never prevailed *epidemicall*y there between 1776 and 1794, although the intercourse with the Havannah and other places where the disease continued to prevail, was quite free. He even says that during the eight years preceeding 1794, “there was not a single example of the vomito, although the concourse of Europeans and Mexicans from the interior, was extremely great, and the sailors gave themselves up to the same excesses which are now laid to their charge.”† Such a fact is the more worthy of notice, as it does not appear to have depended on unusual atmospheric states; and one can scarcely concede any degree of importance to the circumstance of the streets having been, for the first time, paved, in the year 1775,—seeing that the disease has recurred so often since 1794,—and has prevailed so frequently in the well-paved streets of St. Pierre Martinique, of Cadiz, Gibraltar, &c.‡

#### SPAIN.

To turn now to Spain: it appears that so far back as 1761 (21st October), a royal edict was issued at Madrid, which set forth that all experience of the intercourse between the Havannah and Cadiz had proved that the black-vomit fever was not

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\* “If the earth’s surface did not reach to considerable elevations within the tropics, the highly characteristic form of needle-leaved trees would be almost unknown to the inhabitants of the equatorial zone. In common with Bompland, I have laboured much in the determination of the exact lower and upper limits of the region of Coniferae, and of oaks in the Mexican highlands. The heights at which both begin to grow (los Pinales y Encinales, Pineta et Querceta) are hailed with joy by those who come from the sea-coast, as indicating a climate where, so far as experience has hitherto shown, the deadly malady of the black vomit (vomito prieto, a form of yellow fever) does not reach. The lower limit of oaks, and more particularly of the *Quercus Xalapensis* (one of the 22 Mexican species of oak first described by us) is on the road from Vera Cruz to the city of Mexico, a little below the Venta del Encero, 2860 (3048 E.) feet above the sea. On the western side of the highlands between the city of Mexico and the Pacific, the limit is rather lower down, for oaks begin to be found near a hut, called Venta de la Moxonera, between Acapulco and Chilpanzingo, at an absolute elevation of 2328 (2480 E.) feet.

“I found a similar difference in the height of the lower limit of pine woods on the two sides of the continent.”—*Humboldt’s Aspects of Nature*, vol. ii. p. 154.

† Political Essay on the Kingdom of New Spain, vol. iv. p. 194.

‡ Addenda (N.), p. 233.

contagious. It would appear from this that the opinion of the court physician Cervo, sent to inquire into the nature of the Cadiz epidemics of 1730 and 1731, had been more regarded than that of Navarette, who attributed their origin to importation from America. It seems curious that Arcjula, when he wrote in 1806, should have committed the error of supposing the black-vomit of the Havannah, Vera Cruz, &c., to be a different disease from Yellow Fever. He appears in the dilemma of contending for contagion and importation under one denomination (*Yellow Fever*), while he admits that "our ships never brought the germs of the black-vomit, even though they had the disease on board when leaving our possessions;" and (in America) "a succession of ages proved to the medical men that the disease was not communicable." This physician, with Drs. Coll and Amellor, also of Cadiz, made a declaration that the medical men commissioned to inquire into the causes of the epidemics of 1732 and 1734, pronounced it not to have been propagated by contagion. In all subsequent epidemics, a majority of the Spanish practitioners have favoured the doctrine of importation and contagion; but it would appear from the assertion of Professor Salva of Barcelona, in his *Trozos ineditos*, that the opinions of some had been influenced by political or other causes, for he does not hesitate to state, that when he applied for information, private opinions were obtained, in some instances, differing from those which had been publicly declared. A commission, instituted at Cadiz to inquire into the origin of the epidemic of 1810, declared that in none of the six outbreaks which had occurred previous to 1805, could the origin of the disease be traced.\* The importation of the disease alluded to by the Commission as having taken place in 1802, had reference to the disembarkation of about 500 cases from the fleet of Admiral Gravina on its arrival from the West Indies. They admit that though many of the cases had the most characteristic symptoms, and though the communication with the city was free, "the disease did not spread, nor was it in any way communicated." It is also stated that though many cases were sent to the Aguada Hospital at Cadiz, from a French fleet, in 1807, with which a free communication was permitted, the disease did not spread.

Many details connected with the origin of Yellow Fever epidemics in Spain up to the year 1820, are to be found in Hurtado's "*Nueva Monografia*;" in his "*Decadas*;" in Mr.

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\* "En ninguna de estas epocas, exceptuada la de 1805 en que vino de fuera, se ha podido averiguar con exactitud, el origen de esta calamidad publica." "At none of these periods, except 1805, in which it was imported, could the origin of this public calamity be determined with exactness."—*Extract from the Report of the Commission.*

Doughty's book;\* in the writings of Dr. Pariset; and in various pamphlets published since 1827 by Dr. Chervin.

The terrific epidemic at Barcelona in 1821, gave a new impulse to the question of contagion. The statements furnished by the medical Commission sent from France to make researches into the nature and origin of that disease, left a strong impression on the minds of many in the profession, favourable to its possessing a communicable property; and the "*Histoire Médicale*" displays literary powers of a high order on the part of Dr. Pariset, who was at the head of the Commission. Dr. Chervin, however, followed Dr. Pariset, step by step,—not only at Barcelona, but through all parts of Spain where circumstances had been detailed respecting the propagation of the disease.

The result has been,—such a collection of documents as had never before been laid before the public on any question of this kind. As elucidating a long-pending question of high importance to society, their value may be judged of from the opinion of the Academy of Medicine, which has been already referred to in the introduction to this Report. I regret that space will not permit my furnishing many valuable extracts from the works of this gentleman, published in 1827 and 1828.† We are there furnished with the statements of Dr. Pariset and others regarding the yellow-fever epidemics of Spain; and it cannot but be admitted that Dr. Chervin has shown many inaccuracies had crept into those statements, and that the events warranted conclusions quite opposite to those which had been drawn.

Dr. O'Halloran, who went to Barcelona to observe the epidemic of 1821, had, previously to Dr. Chervin's visit, pointed out some of the extraordinary mis-statements of Dr. Pariset; and in his book some interesting statements are furnished relative to occurrences at other points in Spain.

At the same time with Dr. O'Halloran, the late lamented Dr. Charles Maclean visited Barcelona for the special purpose of examining all the circumstances connected with the above-named epidemic.

Many who have studied Dr. Maclean's works, are of opinion that he was rather extravagant in some of his views, but the writings which he has left on the subject of this epidemic are quite sufficient to place him in a distinguished light amongst the profession.

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\* "Observations and Inquiries into the Nature and Treatment of the Yellow or Bulam Fever."—London, 1816. Mr. Doughty served as staff-surgeon in our army at Cadiz in 1810.

† In the "*Revue Critique*," by Dr. de Fermon of Paris, printed in 1829, a *resumé* of the occurrences here spoken of may be found.

On the breaking out of the fever the population of Barcelona was computed at 120,000. Within the walls of the town were found medical men, who, disregarding the evidence of their senses, closed the door against all evidence unfavourable to contagion,—adhering only to tradition. Indeed being often threatened by punishment from Government on the one hand, and by popular tumult on the other, they are obliged to yield to the most absurd errors, from which humanity becomes liable so cruelly to suffer.\*

But, from a certain number of the local medical men at Barcelona, and of the foreign physicians then assembled, a goodly company was formed, resolved to show loyalty to the profession. From among these, a manifesto was issued, signed by ten Spanish physicians, two French, two British, and one American, to the effect that the disease was not contagious.†

Dr. Piguillem, who states that he had been considered as the “prince of contagionists,” was amongst the number of those who subscribed; having (together with 13 other Barcelona practitioners, who then also changed their opinions), adopted non-contagionist views some time after the disease had prevailed.

Those who are interested in the subject may derive much information from perusing the work of Dr. O'Halloran, p. 67.

An event remarkable in the history of Yellow Fever, occurred

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\* Extract from “O'Halloran on the Yellow Fever of the South and East Coasts of Spain,” p. 110.

“Origin of the Yellow Fever at Tortosa in the year 1821.

“The disease, according to the latest official reports of the authorities and Board of Health, appeared in the person of Salvador Curto, a soap dealer, who sailed from Barcelona on the 1st of August, and arrived at Tortosa on the 4th.”

At p. 113, Dr. O'Halloran says. “The first marked case at Tortosa appeared in the person of a seaman, named Buena Ventura Puich, belonging to the brig Ventura. After his entrance he held no communication with Salvador, nor with any of his family. When black vomiting, and other unequivocal symptoms of a malignant disease made their appearance, he was forcibly taken from his bed at eight o'clock at night, obliged to dress himself, and, with a rope around his neck, forcibly dragged by four armed soldiers, to a lazaretto in the country, at which he arrived after a march of three hours. The lazaretto, being unoccupied, was forced open, and the unhappy and helpless victim, without assistance, or even water to allay his thirst, was left to his fate for the remainder of the night without a bed on which to repose, without an individual to whom he could speak, and without nourishment of any kind. He died on the 15th, with the usual symptoms of Yellow Fever, viz., yellowness, hæmorrhages, black vomiting, &c.”

At page 117 he says, “The unexampled cruelty of the Board of Health towards Buena Ventura Puich was not suffered to pass unnoticed. The military governor, Don Mignel de Haro, supposing that its conduct was a species of refinement on brutality, not sanctioned by laws either human or divine, remonstrated against the proceedings, and preferred a formal complaint to the Political Chief of the province, who, in all probability, treated it with little attention; for the propriety or non-propriety of treating a fellow-creature, while under disease, with such brutality, has not hitherto been a subject of investigation in the province of Catalonia, at least if I am informed correctly.”

† Addenda, (O.), p. 233.

at the port of Passages in the province of Guypuscoa, a place known to many British officers, it having been the rendezvous for transports while our troops occupied the Pyrenees in 1813-14. It is difficult to give an idea of this singular port, situated at the bottom of the Bay of Biscay, and forming a sort of appendage to St. Sebastian's. The entrance is between precipitous rocks, and is so narrow and oblique as to be with difficulty discovered. The town consists of one small street, placed on a shelf of rock, so narrow that it does not admit of the passage of carts, or, I believe, horses, while the base of the mountain of Olearsó, is almost in contact with the houses, which are badly ventilated, filthy, dark, and crowded.

We are informed by Dr. Arrutti, a physician resident in that part of the country, that, in June, 1823, a brig, named "Donostiarra," sailed from the Havannah with a clean bill of health; and that, having lost one man on her voyage (from ordinary disease, as far as was known), she obtained pratique at Corunna, after ten days' quarantine. She subsequently put into St. Andero, and arrived at Passages on the 3rd of August, with all on board healthy. This vessel had been employed in the trade of these ports. As she had been at Corunna and St. Andero, previous to her arrival at Passages, she was not here put in quarantine.

The cargo, consisting chiefly of sugar and tobacco, was discharged; and, for several days, a great many people went on board without any disease having broken out either amongst them or the crew. On the 15th, a custom-house officer, who had been several days on board, was taken ill, and died on the third day, black-vomit having appeared. This man was said to have been much engaged in the hold looking after contraband goods. On the 22nd, a man who had been in the hold, surveying the ship's timbers, likewise died. Some of the planks of one of this vessel's sides having been found greatly decayed, 12 carpenters were employed in removing them, and six of the 12 were attacked in quick succession. The opening in the ship commenced on the 19th, and, on the 23rd, the disease began to appear, in an unequivocal form, in the houses close to which she was moored.

Dr. Arrutti proceeds to show that the disease did not extend beyond the houses opposite the ship; that where others were attacked whose habitations were at a distance, it was occasioned by their having remained for some time within the space to which the malaria from the ship appears to have been limited, and the names and occupations of those persons are given. The heat was excessive,  $28\frac{1}{2}^{\circ}$  Reaumur (about  $96^{\circ}$  Fahr.), and the course of the wind favoured the conveyance of the noxious emanations from the ship to the houses: he gives the number of each house in which persons were attacked, and names

the points to which individuals labouring under the disease were conveyed, stating that notwithstanding the adjuncts of *crowded, filthy, and badly-ventilated habitations*, the disease did not spread; "whether they died or recovered, to none out of the focus was the disease communicated."\*

He observes, "The inhabitants of Passages took the precaution of not making long delays in the focus of infection;—they visited their relatives and friends, and performed towards them all the rights demanded by humanity and society, and the disease became extinct almost in its very origin.† It therefore results that this fever, examined according to the character it presents, does not offer a character of contagion from individuals. It was afterwards discovered that many, evading the sanitary regulations, passed out without certificates of health, and took with them clothes, even from the houses where people had died; but, notwithstanding this, there was not the least spreading of the disease in the neighbouring country. If any deaths took place in Loyola, Renteria, or elsewhere, the disease in such cases had been contracted within the focus of infection.

"If this disease had been transmissible by individual contact, what could have put a stop to its progress?—no human power: for the people who had been in the closest contact with sick, convalescents, and clothes belonging to the sick, distributed themselves, when the cordon was about to be placed, at St. Jean de Luz, St. Sebastian's, Bayonne, and other places."

Here then was an instance—not of Yellow Fever imported—nor, rigidly, of *the cause* of Yellow Fever imported, but a development of the disease by the concurrence of a number of agents. On other occasions, it has been observed not to break out until vessels had been cleared of their cargoes; and in this instance the great heat, reverberated as it must have been from the rock close to which the lightened ship was moored, may be easily understood as having been highly favourable to the extrication of a noxious principle from her decayed planks.‡

In another account of this outbreak, by Dr. Montes in the 14th vol. of *Hurtado's Decadas*, its origin is attributed to sources within the town itself, independent of the ship, as publicly de-

\* Page 70.

† It appears that the ship was burned and sunk.

‡ The origin of this disease at Passages, from sources on board, has been admitted by Dr. Andouard of Paris, a professed contagionist.—See "Revue Médicale," Sept. 1824, p. 83.

This gentleman, who carried his views of contagion even to intermittent fever, was a physician in the French army in that part of Spain, and cordons were of course not forgotten; but in advocating his opinions on the occasion he says ("Revue Médicale," vol. iii. p. 224), "Nevertheless it must be confessed that the contagion at the Port of Passages was not so manifest as at Barcelona;" and leads to the conclusion that the first circumstances connected with this outbreak went to prove contagion, the last, the contrary. So that on the whole Dr. Arrutti's statements must be received as those most worthy of credit.

clared by Dr. Zeubeldia; and we are told that a similar disease prevailed there in 1780 and 1791.\* There is no discrepancy, however, in the statements as to the disease not having been propagated from person to person; though Dr. Arrutti seems to apply the word contagion † to the extension of the disease within the limits of the noxious emanations from the ship.

#### AFRICA.

In the same year in which the Fever took place at Passages, another circumstance occurred, which has been frequently alluded to.

By an official Report, drawn up and published in 1824, by Dr. (now Sir William) Burnett, one of the Commissioners of the Medical Department of the Navy, it appears that in the early part of the preceding year a fever made its appearance at Sierra Leone in a form different from the usual remittents of the country, and stated to possess symptoms characteristic of Yellow Fever. The alleged importation of this disease by the merchant-ship “*Caroline*,” is completely refuted in that Report; and at page 24 an extract from the gentleman at the head of the medical department at Sierra Leone is given, in which it is stated, that from all the evidence which could be procured in the colony, there was reason to conclude that the disease was *non-contagious*. A curious circumstance, to which there is perhaps no parallel except that which occurred under my own observation in the West Indies, is related by this gentleman, viz., “that European females and children were perfectly exempt.”

On the 23rd December, 1823, a statement was circulated by Sir Gilbert Blane, calculated more than any thing which had previously appeared to prove the importation and subsequent communication of Yellow Fever. It appears that the “*Bann*,” sloop-of-war, left Sierra Leone for the Island of Ascension at the latter end of March, 1823; that a malignant fever prevailed among her crew, during and for some time after the voyage; and that, on the eighteenth day after her anchoring at Ascension, a disease alleged to have been similar, and in some instances

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\* M. Bally, in his Report on the Yellow Fever at Passages, in 1823, states, pp. 5, 6, that similar epidemics were said to have prevailed there in 1780 and 1791 after arrivals from the West Indies, and also that the French troops in 1808-9, and the English in 1813-14, suffered much from fever at the same place. “Here,” [in the latter instance] “there were no suspicions as to the American typhus; it was the typhus of Europe, that destroyed a great many victims in the hospitals.”—See Addenda, (P.), p. 235.

† I had been long acquainted with Dr. Bobadilla, at Gibraltar, and considered him a believer in contagion, in the sense of direct or indirect transmission from one person to another; but, to my surprise, he assured me that, at an hospital in Los Barrios, some years previously, he explained to everybody how the attendants on Yellow Fever patients were not more liable than others to attacks.

accompanied with black vomit and yellow skin, broke out in the garrison of that island which consisted of 35 men, besides women and children.

It appears by Sir William Burnett's Report, that an error had crept into Sir Gilbert's statement regarding the health of the crew when she left Sierra Leone; but what is of much greater importance to the argument we find altogether omitted by the latter, though supplied by the former, viz., "On reference to the journals of medical officers who, at different times, had charge of the garrison before the appearance of the late epidemic, not only has dysentery and hepatitis been very prevalent, as well as occasional attacks of fever, but likewise a fever called the bilious remittent,\* in the year 1818, attacked almost every man on the island, which the assistant surgeon attributes to an unusually wet turtle season, when the men are much exposed by watching at night to turn these animals.

"Moreover there is, in the journal of Mr. Robert Malcolm for 1818, a case of this disease, which commenced on the 1st of June, and terminated by death on the next day, with all the symptoms of yellow suffusion and black vomit, &c., which are said to characterize the Yellow Fever; and having shown this case to the surgeon of the 'Bann,' now in London, he declares it to be exactly similar to the cases of fever which lately proved so fatal in the 'Bann,' and amongst the marines at Ascension." †

Here then, whatever might have been the nature of the disease which prevailed in the "Bann" and at Ascension, in 1823, we have evidence of the existence of the same disease in the island, and about the same time of year, in 1818, without the remotest suspicion of its having been then imported.

This, on the obvious principle that what may in one year happen on a small scale, may, from an extension of the cause, happen on a larger scale in another, greatly enhances the force

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\* It seems remarkable that though perfectly known for some time past that the true Yellow Fever has been prevalent this year (1850) on the coast of the Brazils, (Bahia, Pernambuco, and Rio Janeiro), and it being also ascertained by returns to Somerset House for the quarter ending 31st March, from H.M. ships "Cormorant," "Tyne," and "Crescent," that most malignant cases (*Yellow Skin, Black Vomit, Hæmorrhages*,) prevailed in each of these ships—the term *Yellow Fever* does not once appear on the official returns;—they all appear, under the head of "Bilious Remittent," so clear, I presume, do the remissions appear in those latitudes. If we consult the army statistics of the mortality among the troops in the West Indies over a period of 20 years, the adoption of the same classification at Jamaica will also seem remarkable, for while we know, that within the period mentioned, the troops in that island have suffered more than in any other of the West Indies, TWENTY CASES ONLY of Yellow Fever appear on the face of the returns during the 20 years; the convictions of the various medical officers stationed there, leading them to a belief that the term Bilious Remittent, so frequently used in different parts of the world, might better convey some of the prominent and more constant phenomena of the disease.—See Appendix. No. III., p. 282.

† Page 10.

of the concluding part of Sir William Burnett's sixth position, "that a disease similar to the fever in the 'Bann' might have prevailed in that island though the 'Bann' never had any communication with it."\* He tells us that "the principal medical officer at Sierra Leone has come to the same conclusion in his official Report;" and I suspect that, greatly augmented as the facts bearing upon the question of contagion have been within the last few years, the majority who have paid attention to Yellow Fever will come to the same conclusion, rather than admit as a *propter hoc* that which, as far as the evidence goes, was simply a *post hoc*.

Sir William, though favourable to the belief of importation on this occasion, leaves the question open, and furnishes all the details to enable the profession to form an opinion. He points out erroneous statements as to the health of the island from the period of our occupying it (1815) to the epidemic year, 1823. He says, "Out of 130 cases of disease which are recorded in these journals, 12 died and 19 were invalided; and though perhaps all the fatal cases are inserted in the journals, it is well known that those documents seldom contain more than a third of the cases which actually occur." He tells us that, according to the medical gentlemen in charge of the garrison, although at a period immediately preceding this event, "they were on the whole very healthy, yet they were by no means exempt from disease."† He admits, "that after a most careful inquiry, it is impossible to trace the fever in question directly from the 'Bann,' to any individual of the garrison of Ascension;"‡ and indeed it appears that the first person attacked was not one of those known to have been in the ship or in contact with the sick, but a boy, respecting whom "it is neither known nor believed that he had any nearer communication with the sick of the 'Bann' than passing daily at no great distance from the tents to feed his father's poultry, and he was never on board that ship."§

The tents alluded to, about 500 yards from the garrison, were occupied as a hospital for the accommodation of the sick from the "Bann," and *all intercourse was interdicted*. He informs us that the restrictions on the intercourse between the ship and the garrison had not been much attended to, several persons having been on board after the landing of the sick; but that, from the time of the boy's illness, "every proper precaution was taken for preventing the extension of the disease to the outposts; notwithstanding which, six men, two women, and seven children, were taken ill at Springs, but fortunately none at the Green Mountain, though one of the men belonging to that post had been on board the 'Bann.'"

\* Page 11.

^ Page 52.

† Page 53.

§ Page 14.

I shall only add that the surgeon of the "Bann," "an excellent and intelligent officer," ascribed the disease to the long stay of the ship in port at Sierra Leone, where the crew had been much exposed to the sun's rays in refitting her rigging, &c.; and that when this vessel had been on the Jamaica station, in 1821, a fever, with yellow skin and black vomit, appeared among a party of 40 prisoners, put on board for a passage;\* which disease, according to the surgeon's journal, did not extend to the crew, nor could he "trace a single instance of disease to contagion," although "the smallness of the vessel, and other circumstances, would not admit of a separation between the sick and ship's crew."

That there were, on a *prima facie* view of the irruption of the fever in Ascension, reasonable grounds for *suspecting* the agency of contagion, there cannot be a doubt; but it is far from established by evidence, that the one was the cause of the other; and many will probably join in believing that to admit the transmission of this disease from individuals, would be illogical, and nothing short of admitting that to be *vrai* which is shown to be only *vraisemblable*.†

### SHIPS.

Considering the mass of evidence before the public, it would be quite idle to deny the spontaneous breaking out of Yellow Fever on board of ships in various situations: one of the best authenticated instances is that of the "Bedford," in Gibraltar-

\* Page 47.

† The outbreaks of Yellow Fever on the Western Coast of Africa cannot be passed over without reference to the work of Mr. James Boyle, R.N., M.C.S.L., colonial surgeon to Sierra Leone, entitled "Account of the Western Coast of Africa," London, 1831.

This gentleman's book displays great merit in several respects. We have from him, speaking of the epidemic of 1829 at Sierra Leone, that "Peculiar season is the immediate cause of epidemic fever;"—"that occasional cases of Yellow Fever are to be met with every year in Sierra Leone;" (230),—"that all fatal cases were under the influence of one general cause, *malaria*;"—(268), "that it appears that in Sierra Leone the disease was not contagious;"—"and that although very many hundreds of individuals, from various parts of the colony, visited the market of Freetown every day, not one case of the disease occurred in any of the villages."

Mr. Boyle (at 238) refers to what he considers the unfounded nature of the report of the Yellow Fever of 1829, being imported into Sierra Leone by H. M. ship "Eden," though he seems to admit that the disease might have been contagious on board that ship.

And, at p. 266, we have the following copy of a document which was posted, by direction of the Governor, in various parts of Freetown:—

"Freetown, Sierra Leone, May 27, 1829.

"WHEREAS a report has been in circulation throughout the colony that the fever now prevailing is of a contagious character, His Honour the Lieutenant-Governor, has deemed it expedient to call for the opinions of the principal medical officers upon this subject, and they, whose names are hereunto affixed, do declare, that there is not the slightest ground for the rumour that contagion exists in the colony.

(Signed)

"J. BOYLE, Colonial Surgeon.  
M. SWEENEY, M.D., Deputy-Inspector  
W. FERGUSON, Surgeon, R.A.C.C."

Bay, in 1794, of which there are official records at Somerset House.

In that year Yellow Fever was not prevalent in the garrison, and the crew arrived in health from the Mediterranean on the 24th of August. On Sunday the 6th of September, the crew having been mustered, every man answered to his name; but in the course of the week 130 were sent to the hospital, with fever possessing the characteristic symptoms: 11 died before the 24th of September, and others were left dangerously ill on the departure of the ship that day.

In this case the only feasible cause assigned was the shifting of the shingle ballast, with the object of trimming her. The disease did not extend beyond the crew.

The fact of the sailors of the British ship "Carnation," when put on board the French brig "Palinure," by which she was captured, in 1808, having been attacked with the disease, would admit of the explanation of their having, in common with the previous cases in the "Palinure," originated from sources within that ship.

The following is among the more recent instances of the spontaneous irruption of black-vomit fever on board ship.

H. M. ship "Blossom" had been employed in the summer of 1830 in surveying the Honduras coast; and in the month of August the disease commenced, which obliged the captain to go into Belize harbour, to obtain medical assistance from the garrison. Forty-eight cases were received into the military hospital between the 11th and 30th of August: two officers and eight men died, and "these cases were attended with black vomit," according to assistant-surgeon Watts, who forwarded notes of the circumstances in his official return to the Army Medical Board 24th September, 1830. Mr. Watts adds that the disease did not extend to the other ships, or to persons on shore.

In a report from Dr. Lindsay, surgeon of the "Blossom," to his department, he says, "I am of opinion that the cause of the present illness arose in the ship herself."

Among the naval surgeons of practical knowledge in this disease, I do not find many supporters of the doctrine of contagion. The following is from Mr. Mortimer, while serving as principal naval medical officer at Barbadoes: "We do not allow the fever of the West Indies, commonly called 'Yellow Fever,' to be at all infectious in any of its forms or stages. We have never known of an instance of its communication to patients at the several naval hospitals, whilst under cure for other complaints, though such patients have never been interdicted, on the contrary encouraged to offer every additional aid for the greater comfort of their suffering brethren." \*

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\* See Communication to Commissioners of Transports, Med. Chir. Rev., vol. viii.

Among writers on Yellow Fever Dr. John Wilson, R.N., is deservedly celebrated. The following instance of British cruelty or ignorance, contrasted with Danish generosity, is recorded by him; he says,\*—

"We are told by the advocates of contagion that measures restricting or preventing intercourse never can do any harm. In the following case—a case which I could scarcely have believed to have happened at Barbados in 1819—it is pretty clear that it did do harm.

"In October, 1819, Her Majesty's ship 'Euryalus' anchored at Barbados from Bermuda. She had a number of fever cases on board, and two men died after her arrival at Carlisle Bay. So strongly did the fear of contagion operate on the minds of men in power, that the hospitals were shut against the sick of the ship. It was intended to have taken measures for putting her in quarantine, but the safer method was *wisely* thought to be that of getting quit of her altogether. She accordingly went to sea, the sickness increased, and she put into the Danish island of St. Thomas, where the governor, actuated by the ordinary feelings of humanity, unfettered by prejudice, gave orders for the immediate reception of the sick into hospital. Seven men were landed at first, and numbers afterwards as they became affected. They were treated by Danish physicians; most of them recovered, and when the work of kindness was completed, they returned to their ship. It was never believed or alleged that they communicated disease to a single person on shore. 'Look on this picture, and on this.'"

On a parallel with the foregoing were the disgraceful occurrences which took place with reference to the "Eclair" from Boa Vista in 1845; when the sick were denied an asylum out of the reach of the malignant poison which was destroying them, even on our own shores.

It cannot be expected that all instances of the appearance of Yellow Fever on shipboard, should be noticed. In 1726 great havoc was made in the fleet of Admiral Hosier, lying off Portobello, by a disease alleged to have been Yellow Fever. In 1741, Admiral Vernon's fleet suffered from it off Carthagena, (S. A.) In 1742, the disease broke out in the same fleet off Portobello. In 1776, the Spanish ships "Angel" and "Astrea" suffered from the disease on their way to the West Indies. It broke out in the squadron of the Spanish Admiral Solano, in 1783. In 1785, in the Spanish ship "San Ildefonso." In 1793, in the "San Lorenzo," one of the ships of the Spanish Admiral Aristobal, bound from Cadiz to the West Indies. In the same

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\* Memoirs of West Indian Fever, 1827, page 77. See Addenda, (Q), p. 236.

year in the squadron of the Spanish Admiral Borja. In 1794, on board H. M. ships "Bedford" and "Kent." In 1795, on board the "Hussar" frigate on the American coast. In 1801, on board the ship "Penelope," carrying Irish emigrants to New York. In 1802, in a French fleet from Tarentum, bound to St. Domingo. In 1803, on board the "Hibbert" on her passage from Portsmouth to New York. In 1802, in the fleet of the Spanish Admiral Gravina. In 1807, in the "Phebe" in the West Indies. In the same year, in a French squadron in the bay of Cadiz. In 1808, on board the French brig of war, "Palinure." In 1813, in an English vessel which arrived at St. Domingo from England, as stated by Dr. Pinedo. In 1814, in a flotilla of Spanish revenue cruisers ("*guardacostas*"). Since this period it appeared in the following ships of war, on the West India station,—"*Iphigenia*," "*Wasp*," "*Tribune*," "*Sapphire*," "*Scout*," "*Tamar*," "*Bustard*," "*Thracian*," "*Rattlesnake*," "*Lively*," "*Isis*," "*Seylla*," "*Pylades*," and "*Ferret*," and many others, particularly in the "*Eclair*,"\* "*Growler*," and other steam ships.

#### EFFECTS OF MIGRATION.

The following proofs are given from Dr. Chervin of the advantage of migration from localities where Yellow Fever takes place.

In his answer to Dr. Lassis, Paris, 1829, p. 13, he shows that the good effects of migration have been repeatedly displayed.—

"During the epidemic of 1800 at Cadiz, 14,000 persons left that city when the disease became suspected.† These people fled to the country, where they remained free from the epidemic,‡ while of the 57,499 who remained, 48,520 were attacked and 7,387 died.

"On the 1st September, 1803, the population of Malaga was 51,745, of which 3,730 migrated and escaped the reigning malady, while out of 48,015 who remained in the city 16,517 were attacked, of which 6,884 lost their lives.§

"In the month of July, 1804, the population of Malaga was 36,008, exclusive of the military garrison, prisoners, and other persons. Of these 4,548 fled, and thereby secured them-

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\* See Addenda, (R.), p. 237.

Much valuable information in reference to the spontaneous production of Yellow Fever on board ship is furnished by M. Forget in vol. x., *Medicine Navale*, Paris, 1832, p. 195, et seq.

† Arejula Breve Descripcion de la Fiebre amarilla, p. 434. "Yo conozco un solo medio seguro y eficaz de libertarse del contagio, que es irse pronto lejos, y volver tarde," p. 339.

‡ P. 230, loc. cit.

§ Salva's *Trozos inéditos*, p. 12.

selves from attacks, while of the remaining 31,460—18,787 were attacked, of whom 11,486 died.” \*

“While these things occurred at Malaga similar circumstances took place at Alicant, where the inhabitants amounted to 13,957; 2,110 left for the neighbouring country, where they remained free from attacks, while of 11,847 in the focus of the disease 6,971 suffered and 2,472 died.†

“All who know the Spanish peninsula must be aware of the exposure to privations under which emigrating populations must suffer in calamitous times. Yet, in these cases, they obtained security from the pestilence by which their neighbours who remained behind were swept away.

“At 20 other places besides those which I have mentioned, similar occurrences took place with similar results.

“While the Yellow Fever epidemic afflicted the inhabitants of Tortosa in 1821; 5,000 persons departed for the villages of Roqueta and Jesus, on the other side of the Ebro, and the neighbouring country, where they remained free from the disease which carried off about 5,000 within the walls of Tortosa.‡

“On the same occasion the inhabitants of Aseo and Méquinenza, both situated on the Ebro, were also preserved by establishing themselves under tents and olive trees, and though suffering privations, those who were at a distance from the river were not attacked.§

“At Palma, in the island of Minorea, the seat of a terrible epidemic in 1821, had migration not taken place the results would have proved much more calamitous.

“Let us hear what the medical commission, which went to Barcelona, said upon the subject.

“On the 15th September the superior authorities, civil and military, left the town for Valdemosa, a village situated three leagues from Palma. The citizens following their example made a precipitate retreat to the country beyond the walls, and the migration was so considerable that of 32,000, 12,000 only remained. It was necessary to raise funds, secure means of maintenance, and build huts in the open country, and two encampments were formed at the foot of Mount Belver, half a league from Palma.

“All the healthy poor were assembled, all who, for want of work, were starving, and all who had strength removed thither, and thenceforth security was found from the disease;” or, as Dr. Pariset expressed it, they escaped the *contagion*. “The

\* Arejula, 4th table in his book.

† Arejula, loc. cit. 5th table.

‡ See petition to the king from the municipality of that city.

§ Histoire Médicale, &c. (Pariset), p. 61.

12,000 who remained in Palma, after the emigration of the rest, had 7,400 sick and 5,341 deaths.”\*

“It was calculated that from Barcelona in 1821 about 80,000 persons fled, and, except some who departed with the disease already upon them, or who were on the eve of being attacked, all remained exempt from the reigning malady;† though subjected, as Dr. Lassis well knew, to great privations, for he says, ‘These refugees from Barcelona experienced all sorts of vexations.’ The inhabitants of the neighbourhood, even those of the highest mountains, influenced by the fear of imaginary contagion, adopted precautions of the most arbitrary nature. They might have been seen exercising towards them measures contrary to the most sacred rights of humanity, and, by such conduct, furnished a deplorable example of superstitious ignorance in which the routine of sanitary [*i. e.* quarantine] measures had placed the people.”

“In spite of all the sufferings of the fugitives from Barcelona they remained free from the epidemic. While, from about 70,000 persons in the city, near 10,000 deaths took place.”‡

“In 1823, when the Yellow Fever broke out at the Port of Passages 3,000 persons took flight, and it was fortunate that this measure limited the deaths there to only 40.§

“On that occasion, as at Barcelona, in 1821, after the flight of the people, a few only, who left the town ill, had the disease.”

On the necessity of migration on the outbreak of Yellow Fever, I will now merely add a few words from the Baron Dupuytren, in his Report to the Academy of Sciences in Paris, 1825.

“We should therefore regard as incontestable the principle which consists of evacuating immediately the places where Yellow Fever is declared to be, and everything for this purpose should be adopted. The utility of such a measure must always justify its rigorous execution.”

#### QUARANTINE LAWS.

After the details given in the foregoing pages, I am not permitted to doubt, but that those who have followed me through them will have arrived at the conclusion that Quarantine Laws, in reference to Yellow Fever, are not only unnecessary, but cruel and unjust,—often producing great privation and suffering,—frequently, instead of checking the progress of the disease, increasing the number of victims by confining people

\* Hist. Med. p. 70.

† Ibid. p. 25.

‡ Ibid. p. 134.

§ Arruti *Tradado de la Fiebre amarilla*, p. 75.

to ships or infected localities on shore, thereby rendering escape from the real source of the disease impossible,—and, finally, acting most injuriously on commercial intercourse. But perhaps I cannot exhibit the necessity for the abrogation of those laws in a stronger light than by giving the following passage from a work on Yellow Fever,\* by Dr. Reider, of Vienna, who made this disease his particular study, and, for this purpose, made several voyages to the West Indies and the American Continent, where, like Dr. Chervin, he was enabled to observe every particular characteristic of the disease. He says, p. 106,—

“All these circumstances are calculated to fill with horror the breast of every feeling and honest man; and we are really obliged to offer violence to ourselves in not giving vent to our indignation against the partisans of contagion, who yet desire to continue to defend their erroneous opinions, and who, to this day, have used all their efforts to make obscure and disfigure the subject, to the great detriment of truth;—who have never ceased to deceive governments, which think it their duty, with regard to this disease, to surrender themselves to the judgment and knowledge of medical men,—who have never ceased to describe it as contagious, and have induced those authorities to adopt, with respect to it, the most false and contrary measures,† and to neglect the suitable, prophylactic, and *preservative* means, and others which might have put an end to the disastrous epidemics of this disease;—thus it is they have always acted contrary to truth, to the interest of governments and of humanity.”

In conclusion, I beg to quote the following extract from a document furnished to the American Commission at Albany, 1845,‡ by Dr. Reece, of New York, who says,—

“I am of opinion that the oppressive features of our Quarantine system should be reckoned among the relics of barbarism which an enlightened Legislature should make haste to abrogate for the sake of our character as a people.

“There is no pretext for the perpetuation of a system founded in ignorance, and fruitful only in public and private injustice, cruelty, and wrong.”

\* *Memoire sur la Fievre Jaune*, Vienna, 1828.

† “Twenty-six persons were detained in quarantine of observation at the ‘Venta’ of the Capuchins of Reinteria, for having communicated with Ajarbé, the carpenter, who died there [of yellow fever] on the 31st August, and the time which had been prescribed by the Sanitary Board of Bayonne having expired without any of these persons having been attacked, I consented to their being set at liberty on the 28th September.”—*Andouard, de la Fievre Jaune au port du Passage*, 1823.

‡ *Addenda* (S.), p. 238.

## ADDENDA.

## LETTER (A.)

*That Yellow Fever prevailed to a serious extent in the 48th Regiment, in the autumn of 1798, appears by the following portion of a letter from the late Staff-Surgeon Hill, dated 13th June, 1832:—*

“The 48th regiment having returned from the West Indies a complete skeleton, were completed by recruits from the different supplementary corps to the number of 1,100 strong, with which it immediately embarked on board the “Caleutta” Indiaman the 1st September, 1798, at Lymington, and arrived at Gibraltar the following month (October); previously to the regiment embarking there had been a detachment on board, and two or three cases of small pox occurred; the ship had been fumigated and whitewashed, but nevertheless a few cases of small pox were debarked at Gibraltar on the arrival of the regiment at that fortress; in all other respects the men were healthy, although much crowded on board ship, and such was the state of equipment that the men appeared in their different county militia uniforms, not having had time to fit on the proper uniform clothing prior to leaving England.

“Soon after the arrival of the corps at Gibraltar several cases of yellow or bilious remittent fever made their appearance, which increased rapidly and proved highly fatal. Dr. Harness, physician to Lord St. Vincent's fleet (afterwards one of the Commissioners of the Sick and Hurt Board), declared the fever to be precisely the same he had seen in the West Indies, and requiring the same treatment; indeed the fever was of the same type, but in a more aggravated form than I have repeatedly [since, J. G.] seen during the autumnal months, particularly when it was the bad practice to encamp regiments on the Neutral Ground under pretence of keeping them healthy.

“It is to be remarked that the young recruits were the only sufferers, not an officer having been attacked, most of whom had lately returned from the West Indies, but amongst the subalterns there were several that had never before been out of England.

“The regiment on its arrival was quartered in the King's bastion, a bomb proof casemate, and not sufficient to properly accommodate half the battalion, the hospitals were equally bad and crowded.

“The other regiments in garrison did not suffer from fever more than usual, and the disease was confined solely to the 48th, and no apprehension or dread was entertained of the fever spreading either amongst the military or civilians, neither were there any precautionary measures resorted to, for in those days the antisocial doctrine of ideal contagion was not in existence.

“ (Signed) WM. HILL,

“ *Deputy Inspector of Hospitals.*”

[ *Vide accompanying Plan of Gibraltar.* ]

The following list of cities, towns, and villages of Spain, in which Yellow Fever is admitted to have appeared since 1800, may not be uninteresting to the profession; but in many of these places the disease was confined to circumscribed localities where the Sanitary conditions were notoriously defective, and did not extend to the mass of the population.

*In Andalusia.*

*Cadiz.*—Built at the extremity of a narrow neck of land, extending about 6½ miles into the sea. Lat. 36. 32. Number of houses 4,020; population 53,500.

- St. Fernando*.—In the Isla de Leon, near Cadiz. Situation low. Population 3,400.
- Puerto de Sta. Maria*.—Situating opposite Cadiz. Low.—Population 17,500.
- Puerto Real*.—In Cadiz Bay, opposite that city. Situation low. Population 5,000.
- Rota*.—On the coast, one league from Puerto Sta. Maria. Population 7,997.
- Chipiona*; lat. 36. 40.—On a rock on the coast, near the mouth of the Guadalquiver,  $1\frac{1}{4}$  leagues from San Lucar and 3 from Cadiz. Population 500.
- Ayamonte*; lat. 37. 13.—At the mouth of the Guadiana, near the frontiers of Portugal, in a sterile country. Population 6,300.
- Medina Sidonia*.—In bishopric of Cadiz, and 5 leagues E.S.E. of that city; 4 leagues from the sea. An old town built on a high hill. Population 9,300.
- Vejer*.—In bishopric of Cadiz, on a very high but cultivated hill, at a short distance from the sea, opposite Cape Trafalgar. Population 8,172.
- Los Barrios*.—A small village; situated in the mountains, about 14 miles W. of Gibraltar, on the road to Cadiz.
- Algeciras*.—Situating in the Bay of Gibraltar, and opposite the latter town; little elevated above the level of the sea; behind it are high ranges of mountains. Houses 2,200; population 9,800.
- Gibraltar*; lat. 36. 6. 42.—Population, including military (and comprising the district called South, as well as the territory beyond the walls near Neutral Ground), 22,000.
- Ximena*.—In bishopric of Cadiz, about 20 miles from Gibraltar, in a N.W. direction; situation on a hill. Population 6,000.
- Paterna de la Ribera*.—A small town situated at the foot of the Xerez mountains, in the bishopric of Cadiz. Population 1,500.
- Xerez de la Frontera*; lat. 36. 41.—Four leagues N. W. of Cadiz, on a hill in a fertile country. Population 31,000.
- San Lucar de Barameda*; lat. 36. 47.—At the mouth of the river Guadalquiver; situation low. Population 16,500.
- Arcos de la Frontera*.—Situating on a very high rock, in bishopric of Seville, 10 leagues north of Cadiz. Houses 3,050; population 10,000.
- Villa Martin*.—Archbishopric of Seville, on the river Guadaleta, 13 leagues from Cadiz and 12 from Seville. Population 2,700.
- Espera*.—Archbishopric of Seville; situation low; near a small river which joins the Guadaleta opposite Arcos, 10 leagues from Cadiz. Population 2,900.
- Lebrija*.—Archbishopric of Seville, in a plain 2 leagues from the river Guadalquiver. Population 6,700.
- Utrera*.—Archbishopric of Seville, 2 leagues from Seville; situated between two hills; streets wide and paved. Population 1,105.
- Alcala de Guadaira*.—Archbishopric of Seville; a small town situated on a hill. Population 5,000.
- Seville*; lat. 37. 24.—On river Guadalquiver; situated in an extensive plain, 24 leagues N.N.E. of Cadiz. Houses 12,055; population 100,000.
- Carmona*; lat. 37. 28.—Archbishopric of Seville, on a hill 6 leagues to the E. of that city. Population 18,000.
- Moron*.—Archbishopric of Seville, at the foot of the chain of mountains called Sierra de Ronda, 10 leagues N.E. of Seville. Population 7,500.
- Ecija*; lat. 37. 31.—Archbishopric of Seville, in a plain on the western bank of the Xenil, 6 leagues before its junction with the Guadalquiver, in a fertile country. Population 34,727.
- Antequera*.—In a plain between Grenada and Seville; it is built partly on a rising ground; streets straight and wide; 26 miles N.N.W. of Malaga. Inhabitants 13,000.
- Cordova*; lat. 37. 52.—Situating in a plain near the Sierra Morena, on the river Guadalquiver; distant from the sea 70 miles in a direct line. 46,500 inhabitants.

- Montilla*.—Archbishopric of Cordova, 6 leagues to the S.E. of that city; situated partly on a hill. Population 12,000.
- Espejo*.—Archbishopric of Cordova; on a plain on the river Guadajoz, 5 leagues from Cordova.
- La Rambla*.—A town in the archbishopric of Cordova. Population 7,800.
- Corlotta*.—Archbishopric of Cordova, 5 leagues S.W. of that city. A large village; population about 2,000; 26 leagues from the sea.
- Aguilar*.—A town in archbishopric of Cordova. Population 11,000.
- Granada*.—Situated near the Sierra Nevada mountains, at an elevation above the sea of 927 feet; about 16 leagues from Malaga, and about 31 miles in a direct line from the sea. Population 80,000.
- Malaga*; lat. 36. 43.—At the base of a range of hills; a very remarkable hill on its eastern side. About 7,000 houses; population 52,000.
- Velez Malaga*.—Situated at about 14 miles to the E. of Malaga, on the slope of a hill, 2 miles from the sea, on the river Velez. Population 14,000.
- Alhaurin*.—A small village on an eminence, within a short distance of Malaga. Population about 400.
- Churriana*.—A small village between Alhaurin and Malaga, situated on an eminence.
- Alhaurinejo*.—Situated about  $1\frac{1}{2}$  league from Malaga; a village.
- Vera*.—Bishopric of Almeria, situated about 3 miles from the sea, in a plain. Population 8,000.
- El Palo*.—A village distant about a league to the E. of Malaga.
- El Borge*.—A small town, bishopric of Malaga. Population 1,200.
- Ronda*.—Situated in the midst of a range of mountains of that name, at a very great elevation above the sea; distant from Gibraltar 60 miles N. Population about 18,000.
- Also *La Carraca*, *Chiclana*, *Las Cabezas*, *Sn. Roque*, *Mairena*, *Estepa*, *Carrana*, *Los Palacios*, *Villafranca*, *El Archal*, *Dos Hermanos*, *Tribujena*, *Bornos*, *Zara*, *Almeria*, and *Ubrique*.
- Total number in *Andalusia*, 59.

*In Murcia.*

- Murcia*; lat. 37. 58.—Situated on river Segura, 9 leagues N. of Carthagen. Population about 35,000.
- Carthagen*; lat. 37. 35.—Territory rather unfertile. Population about 30,000.
- Jumilla*.—Bishopric of Carthagen, 10 leagues from Mureia, at the foot of a hill, 40 miles from the sea and 65 miles N.W. of Carthagen. Population 8,000.
- Molina*.—A small village, 2 leagues from Murcia, in a plain, on the river Segura. Population about 1,300.
- Archena*.—Situated 4 leagues N. of Mureia, in a plain at the foot of a range of mountains near river Segura. Population about 800.
- Ojós*.—Five leagues N. of Mureia, in a narrow valley on the river Segura. Population 300.
- Ricota*.—Five leagues and a half N. of Murcia, in a plain at the foot of a range of mountains, and distant a quarter of a league from river Segura. Population about 1,000.
- Alberca*, *Yelar*, *Alcaria*, *Mazarron*, *Las Aguilas*, *Totana*, *Lorea*, *Ziera*, *Villa-nueva*.
- Total number in *Murcia*, 16.

*In Valencia.*

- Orihuela*; lat. 38. 8.—Situated in a plain, near a range of mountains on river Segura, and distant from the sea 14 miles. Population 25,500.
- Guardamar*; lat. 38. 15.—A small sea-port, at the mouth of the river Segura. Population about 4,000.

- Alicant*; lat. 38. 20.—Situated in a fertile country, on the sea-coast. Houses 4,500; population 21,000. At the foot of a high hill.
- Penacerrada*.—A small village, 1 league from Alicant, consisting of 42 families, 158 inhabitants.
- St. Juan*.—A small village in the neighbourhood of Alicant.
- Elche, Alcantarilla, Palmar, Lebrilla, Albama, Tabarca* (a small island). ;
- Total number in *Valencia*, 11.

*In Catalonia.*

- Barcelona*; lat. 41. 21.—Situated in a plain, in an extensive bay, having on its western side a remarkable mountain. Houses 19,000; population about 120,000.
- Barcelonetta*.—Situated near Barcelona, and in the same bay, distant from latter city a quarter of a mile.
- Tarragona*; lat. 41. 7.—Situated at the mouth of the river Francoli, at an elevation above the sea of 760 feet. Population 11,000.
- Tortosa*; lat. 40. 48.—Situated on the left bank of the Ebro, 4 leagues distant from the sea. Population 13,000.
- Asco*.—A small town on the right bank of the Ebro, partly on a hill, 13 leagues from Tortosa. 300 houses; population 1,300.
- St. Eloy, Escala, Torreuela*.
- Total number in *Catalonia*, 8.

*In Aragon.*

- Mequinenza*.—Bishopric of Saragossa, at the confluence of the rivers Ebro and Segra; distant from the sea in a direct line 40 miles. Population 1,469.
- Nonaspe*.—A village near Mequinenza, on the small river Nonaspe. Population about 1,000.
- Number in *Aragon*, 2.
- In *Old Castile* 1,—*St. Andero*.
- In *Guypuscoa* 1,—*Passages*.
- Total number of places in Spain, 96.
- In the great majority of these places the disease prevailed to a limited extent only.

LETTER (B.)

*Copy of a Letter from Dr. Bobadilla to Dr. Gillkrest, November 2, 1829.*

"SIR,

"I HAVE received your favour of yesterday's date, in which you request some details regarding the occurrences in Gibraltar connected with the Yellow Fever which prevailed in the year 1810.

"We were directed by Government to report all suspicious cases; and, agreeably to my duty, I gave in the names to Dr. Pym (whom we had then as chief medical officer) of individuals at three places in the city which I knew of: one of these was in a house opposite the meat market belonging to Mr. Boscchetti; another in the green market, and the third in Bonastero's lane, in the house of Mr. Beloty. I reserved my opinion as to the true character of the diseases at the time, leaving that to him to do in preference; but, notwithstanding this, Dr. Pym came to my house at midnight of the same day, in order to obtain my opinion; and I then declared to him that they possessed the characters of the true Yellow Fever, and he immediately took the steps which I judged advisable, and which we had been in the habit of adopting in Spain, by which, and a north wind which lasted seven days, and which purified the atmosphere completely, the disease ceased, as I had foretold Dr. Pym it would, on the first day of the setting in of the wind.

"I remain yours, &c.,

(Signed) JOAQUIN BOBADILLA."

"A true copy of the English translation in the possession of Dr. Gillkrest.  
HENRY T. MAXTED."

I think it of importance to notice respecting the Yellow Fever at Gibraltar in 1810, stated by the Superintendent of Quarantine as having been cut short by his having placed "a cordon of troops round the infected part of the South District for fourteen days;" that the spot surrounded by the cordon (see p. 30, Pym's 2nd Edit.) was by no means the only one where Yellow Fever cases appeared, for it is shown by Mr. Amiel, that outside the cordon the wife of Assistant-Surgeon Martin died of the black vomit. Cases also appeared at Cooperage Barracks (then plan, Letter P.), among the 7th Veteran Battalion, besides five or six cases at other points.

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LETTER (C.)

*Extract from O'Halloran "on the Yellow Fever on the South and East Coasts of Spain." P. 167, et seq.*

"There are some points in the work of Dr. Pym, to which I must call the attention of the reader. I am sorry to be under the necessity of doing so, but such palpable errors as appear in his work, on subjects which essentially concern the welfare of mankind, imperiously demand that I speak the truth in the manner in which it has impressed me. Were I not to do so, and to do it without reserve, I should hold myself culpable in the extreme. I am ready to answer, under every possible penalty, for the fundamental correctness of what I state; and I now proceed to state it in as concise a manner as possible, leaving the reader to draw his own conclusions. I am responsible for the fact.

"Dr. Pym, at page 55 of his book, [1st Ed.] alluding to the fever of 1813, which prevailed at Gibraltar, states—'Of 500 persons confined to the dockyard during all the time of the sickness, there was not an instance of one of them being attacked, although this was, of all others, the most likely spot for marsh effluvia to exist, and which, during the fever of 1804, suffered equally with other places, in consequence of the communication not having been cut off.'

"[At page 139] 'The labourers belonging to the naval works have been kept in strict quarantine in the dockyard, very near the spot where the disease showed itself in 1810, and if there is a situation in Gibraltar favourable to the generation of marsh miasmata it is there; and in 1804, it shared the fate of the other parts of the garrison; yet those people this year have continued healthy, as well as another party of inhabitants, who established themselves in Camp Bay, and cut off all communication with the infected.'

*Dr. O'Halloran observes—*

"The perusal of the foregoing quotations in the work of Dr. Pym struck me forcibly on my arrival at Gibraltar in the present year. I thought the immunity of the dockyard from fever in the year 1813 a singular circumstance, and one which strongly operated against the doctrine which I am inclined to embrace. I had not doubted the assertions of Dr. Pym and Mr. [W. W.] Fraser; for, from their rank in the service, the one being at the head of the Health Office in London, and the other at the head of the Medical Department in Gibraltar, it was reasonable to expect information of authenticity, for their opportunities of attaining it exceeded that of others. It happened, however, by accident, that a medical gentleman, who saw the epidemic of 1813, observed, in the course of conversation, that fever prevailed to some extent in the dockyard of that year; and that, by an application to Mr. Buck, who was secluded with the others, and who is now the superintending officer in charge of the establishment, particular and authentic information might be obtained on the subject.

"I applied to Mr. Buck, and the information which I have obtained from him and his head clerk is the following; it may be depended upon as officially correct.

"An order was issued on the 18th of September, 1813, for preventing intercourse between the dockyard men and the inhabitants of Gibraltar.

"The whole of the officers and clerks are hereby directed to repair to the

yard this evening, as all communication will cease with the garrison at daylight to-morrow morning.

(Signed) PERCY FRASER, *Commissioner.*†

To the respective Officers.

“DEATHS and CASES in the DOCKYARD at GIBRALTAR, in the Autumn of 1813, from the 18th of September, to the 23rd of December. Taken from the books.

“ *Deaths.*

- “ 1. Marion Thomas, shipwright, sent to the lazaretto, where he died.
- “ 2. The sister of Mr. Pontez, a sawyer, died on the 20th of September, in the boat-house loft. This woman was supposed to have introduced the disease.
- “ 3. Antonio Mattas, (date not mentioned), died of a very short illness in the boat-house. Hæmorrhages from the nose, mouth, &c., preceded his death.
- “ 4. Mrs. Hamlyn, died in the block store.
- “ 5. Juan Mirobas, died 24th of September, on the careening wharf.
- “ 6. Francis Guerero, labourer, died 6th of October.
- “ 7. Antonio Sowsa, died 20th of October.
- “ 8. Herbert's child, died on the sail-loft on the 20th of October.

“ *Sick.*

“ William Walker Harder ; Samuel Fursman ; William Bernard ; Robert Monk ; William Betts, sent to lazaretto ; William Ancel ; William Whitehead ; Patrick Thoneo ; Joseph Caprella ; Rafael Pons ; Antonio Fiel ; Mrs. Canter ; William Salmon ; Robert Newman ; Mrs. Denham ; Diego Dalmuda.”

“ The communication between the sick and healthy was uninterrupted. The number of persons secluded, according to Mr. Buck's account, who examined the books in my presence, amounted to 170. Dr. Pym makes it 500.

“ The people of the dockyard were released from quarantine on the 24th of December, 1813.

“ The above list of deaths, sick, &c., in the dockyard at Gibraltar, during the period alluded to, is, (I blush to say, for the honour of the profession) authentic.\* The fact cannot be doubted, and the truth of it might have been, it is presumed, ascertained by referring to almost any insignificant individual now in the dock, yard. The impressions which are made upon those who are present in the lamentable scenes of sickness, the perpetual dread of the impending attack, the solicitude to avoid it, and the impossibility of avoiding it, as confined within a narrow barrier, may be supposed to have left traces in the memory which are not soon obliterated. It, therefore, could not have been difficult for this head of the medical department to have satisfied himself of the actual fact, in a range so limited as that of Gibraltar ; and thus to have saved his own mortification, and the public evil that may have arisen from his erroneous report. I cannot divine how the error alluded to can be explained, or what can be adduced in extenuation of a mistake so palpably flagrant as this.† The evidence now given is conclusive against the truth of the assertion of Dr. Pym ; and the careless-

\* The dockyard authorities conclude a certificate given to Dr. O'Halloran, 29th August, 1823, “ The truth of the assertion as to deaths having taken place there cannot be doubted, and the list of sick which you have given falls far short of the number affected ; but the dread of being sent to the Lazaretto, and being separated from their friends and relations, caused the sufferers to conceal the disease in many instances.”

† To my utter astonishment I found this statement repeated in the second edition of Sir W. Pym's work (p.34), printed in 1848, though he had been twice in Gibraltar after the error had been thus publicly exposed.

ness with which he made such report, or the motive which induced him so to modify an important fact, that it has no resemblance to truth, cannot fail to meet with reprobation from honest men, whatever be the doctrines which they adopt. It does not belong to me to seek for excuses for other men's errors or oversights; but it may probably be suggested, that the dockyard was not within the control of the military commander, and that this was the cause that the statement was incorrect. This excuse, I may add, if such be made, would not be valid, for some of the sick at least were accommodated in the lazaretto, which was under superintendence, whether they recovered or died."

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LETTER (D).

*Extract from a Letter from Dr. Gillkrest to Dr. Hennen, 4th September, 1828, as to the origin of the Gibraltar Epidemic.*

"As far as human judgment can go, I think that at least a high degree of probability arises out of the evidence, such as I have heard it stated, respecting *the drains*; but, on a question where so much difference of opinion has always existed, it may be well not to leave the question [of its being *imported* by a certain ship] entirely unheeded: and as all such points become matters of history, I may perhaps be excused by you for suggesting, if you have not quite made up your mind, that a medical inquiry of a formal nature should be instituted; I may add, this seems to be the opinion of others."

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*Extract from a Letter from Dr. Smith to Dr. Hennen, on the same subject.*

"At the same time I cannot help thinking if there is the slightest foundation for this belief [in the *importation* of the disease by the Swedish ship 'Dygdén,'] it merits the most serious consideration, and before determining on the origin and nature of the present epidemic, if I may yet call it so, the most minute and impartial investigation ought to be instituted on this important subject. On the decision of this question depends, in a chief degree, the nature of the means which it may be considered advisable to recommend for eradicating the disease, as well as the chance of safety which it holds out to the healthy by a timely and efficient separation from the infected."

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LETTER (E).

*Copy of a Letter from Dr. Hennen, Inspector-General of Hospitals, to Sir George Don, on the appearance of the first cases of the Epidemic.*

"Inspector's Office, Gibraltar,  
August 29, 1828. 9 o'Clock, A.M.

"SIR,

"In reference to my letter of this day's date, I have now the honour to inform your Excellency that I have minutely inspected District No. 24, in company with Mr. Wilson of the Civil Hospital, Mr. Woods, the medical officer attached to that district, and other staff officers, and it is with much regret I have to state to your Excellency, that in almost every step I took in that district, I had reason for surprise, not that fever had broken out there, but that it had not extended farther.

"It would lead me into matters not immediately connected with professional points, were I to enlarge upon all I have seen and heard during my inspection of that district, but the conclusions to which I am irresistibly led are as follows:—and I beg to claim your Excellency's special attention to them, as the only means for preventing a repetition of those horrors which occurred in this garrison at the period of your Excellency's happy arrival in 1814.

"From whatever causes it may have proceeded, the pauper population is dense to a degree incredible, except to those who have seen it. In sheds, without ventilation, without drainage, and generally composed of the slightest materials, in tiers of beds, as close as in a crowded transport, numerous individuals sleep. They go to their work at an early hour, and return at gun-fire, locking up their miserable places of nocturnal shelter during the day, and leaving them saturated with the steams of their bedding, their food, and the overflowing receptacles of their ordure. The detail would be too disgusting to be entered into, but I most respectfully submit to your Excellency the indispensable necessity of sweeping [away] the whole of those sheds, which I have every reason to suppose are unauthorized by the Government, and are solely the offspring of the most sordid avarice; so confident have been the owners of these sheds at their permanence, that some of them are actually covered with sheet iron, a measure which, while it may tend to put money into the pockets of the owners, by preserving the wretched sheds, must especially conduce to render these places hothouses of contagion.

"Might I presume to offer my opinion, a committee of civilians, military and medical officers, should be immediately appointed to inquire into the state of all the temporary buildings through the garrison, and if they are deemed incompetent for the purpose of human accommodation, and with a risk to public health, that they may be forthwith razed to the ground, as I understand many of them have heretofore been, although subsequently from increased demand on the part of the lower orders, for shelter, they have sprung up with incredible rapidity, contrary to the spirit of those admirable police regulations which were laid down by your Excellency.

"Although my recent observations have led me to a positive conclusion with regard to the state of the District No. 24, I cannot doubt that all the other districts are comparatively in a similar state.

"2nd. Without wishing to implicate any individual in the charge of neglect of duty, I would suggest that the police serjeants of districts should receive the most positive orders to attend especially to the duties connected with the cleanliness of the places committed to their care. On this subject I shall only say, that a most respectable medical officer has stated to me, that he rarely sees a police serjeant perambulating his bounds, and in truth the veracity of his assertion was confirmed to me by more senses than one. Under this head I may mention that I had repeated evidence, that your Excellency's orders, with regard to placing barrels, baskets, &c., for the reception of dirt, in commodious situations, for removal by the scavenger, appear in many cases to have been altogether neglected.

"I shall feel it both a duty and a pleasure to accompany any committee that your Excellency may think proper to appoint, to examine into the multiplied causes which at present threaten the public health. I shall lose no opportunity of visiting every part of this garrison, and shall report to your Excellency every circumstance which, in my opinion, may tend to the preservation of the public health.

" (Signed)

J. HENNEN, M.D.

" *Inspector of Hospitals.*"

In his official report for the year 1826, p. 145, Dr. Hennen, speaking of the Epidemic of 1814, says,—

"These cases first appeared on the hill side, considerably above the old nidus of fever (Boyd's Buildings,) many of them occurred at Cavallero's Buildings, situated close to Arengo's Gulley, they lie about the highest of any houses on the Rock, but are now in a very different state to what they were in 1814. At that period Cavallero's rivalled Boyd's for filth, there were neither drains nor necessaries, and the inhabitants consisted of nearly 300 of the lowest order of Portuguese; at the distance of about 150 paces was established the *grand dépôt* of the garrison filth, the stench of which was intolerable, and the swarms of flies that infested the whole neighbourhood almost exceeded belief." Mentioning other points with regard to this part of the town, he adds, "can we then wonder at the appearance of fever here?"

## LETTER (F.)

The following statement was furnished to T. Jones Howell, Esq., by the late W. Sweetland, Esq.

*"Memorandum relative to the Swedish Ship 'Dygden,' H. G. Gerle, Master.*

"This vessel arrived here from Havana on the 28th of June last, after a passage of forty-seven days. She was partly, perhaps one-third, laden only, and had nothing on board but sugar and logwood. She sailed with a crew of sixteen persons, including the captain, all in good health. On the 27th of May a seaman died, and on the 1st of June another, being fifteen and nineteen days after her departure. The clothes and bedding of these persons were thrown overboard with the bodies. The disease of which the men died did not extend itself to any other of the crew, who all continued healthy up to the time of her arrival, which, as above stated, was on the 28th of June, consequently twenty-seven days had elapsed from the day of the last death to that of her anchoring here.

"The ship was visited by Dr. Hennen, the officiating Inspector of Health, who made every necessary inquiry respecting the state of health of the crew, and satisfied himself there was nothing whatever to mark her case beyond that of any ordinary arrival from the West Indies, and she was accordingly placed under a quarantine of forty days, being the period fixed by Proclamation for ships on board which deaths have occurred from fever, and whenever such are not occasioned by accident or by violent means, it is always assumed that fever is the cause of them.

"Soon after the 'Dygden's' arrival it was written from hence to Spain, and also, as I have been informed, to Italy, that she arrived with the Yellow Fever on board, and had lost several men from that cause, in consequence of which information the government of Alicant extended the quarantine on Gibraltar from eight to sixteen days.

"The knowledge of these and other circumstances induced me to put Dr. Hennen on his guard against the omission of any little form, and in consequence the Doctor regularly paid a visit to the 'Dygden' whenever his duties called him afloat. Nothing occurring to excite suspicion of her being unhealthy, at the expiration of twenty days, as directed by the law, the ship was permitted to commence the discharge of her cargo. Two Health Guards were placed on board, under whose superintendence the linen, and clothes, beds, and sails were aired every day, that is, a part of them each day; and the ship and crew fumigated twice. Her quarantine expired on the 6th of August; she was released, and sailed on the 12th of that month bound to Cadiz.

"The injurious reports by which the 'Dygden' had been assailed during her stay here, had preceded her to Cadiz, where she was refused admittance, by reason, as I was informed, of her having lost men while under the performance of quarantine in this Bay; nor was the impediment removed but by the production of a certificate from hence in contradiction of the report.

"It was then stated, that the cause of the refusal of the government of Cadiz to admit the ship was, that she had arrived there in a sickly state. To this was opposed an official certificate from thence of the perfect state of health on board her at that period.

"It may, perhaps, be unnecessary to record any of the minor reports which were propagated to the prejudice of this unfortunate ship; suffice it to say, the greater part of them were contradicted, by those even who were said to be the authors of them; and many were so exceedingly absurd as to need no refutation. People, I imagined, were tired of the subject, and many days passed without my having heard anything about it.

"About the latter end of August, or very early part of September, a fever appeared in the upper part of the town, as was said, near a drain, which was

stated to be choked and to emit a pestiferous stench, declared by the principal medical officer to be quite sufficient to induce such a disease without other agency, and this opinion was espoused by many gentlemen supposed to be very capable of judging of its probability. The general belief was, however, that the disease had not originated here, and this doctrine was taken up by the whole mass of the population, and facts were eagerly sought for to demonstrate the importation of the fever.

"It was now whispered about, that six men had died on board the '*Dygden*' during the performance of her quarantine; that one of the Health Guards, on landing from the ship, had carried the fever to his house, which was situated in the infected district, where he had died; and that his sister, who had washed his linen, was lying at the point of death under an attack of the same disease. These persons being perfectly well known in the town, their appearance was a sufficient contradiction of the tale.

"It was next stated, that a woman who had washed the captain's linen had been one of the earliest victims to the pestilence, and that she, and not the sister of the Health Guard, had washed the clothes of the latter. This story obtained credit for a time.

"It was, I think, nearly a month after the first appearance of the epidemic, that the Health Guard here spoken of was attacked by it; his case was among the worst which had occurred; he considered himself, and was so considered by his medical attendant, to be on his death-bed, and under that impression he made a declaration to this effect, viz.:—

"That on going on board the '*Dygden*' he and his companion found the crew to consist of fourteen persons, the master included, all in perfect health. That they were most particular in the observance of their instructions by reason of two men having died on board [on the voyage]; causing the sails to be loosed and aired, and the clothes of the captain and crew to be aired every day during their stay, which was eleven days, causing the hammocks and clothes of the seamen to be washed. The most perfect health was enjoyed by every one, the men attending every day at work, and at meals eating heartily. That they caused the ship to be fumigated twice, together with the captain and crew. That he, the declarant, landed on the 6th of August, and the captain having expressed a wish to buy some neckerchiefs, he accompanied him to a shop where the purchase was made, and from whence the said neckerchiefs were taken to the Health Guard's house, and made by his sister, who lived with him. And he concludes his statement by a declaration that the woman who had washed his clothes was in good health, and lodged near him in the Civil Camp, from whence his declaration is dated.

"The above statement was confirmed in all its parts by the Health Guard on his recovery, fully corroborated by his companion; and this very important fact stated by both, 'That it was the custom on board the '*Dygden*' for the crew to wash their linen twice a week, and that the captain had a handy servant, who not only washed but even ironed his linen; that he had no linen washed here, and when asked if he wished a laundress declined having one, saying he would have his linen washed in Cadiz, where washing was cheaper;' so that there can be no doubt of his not having had any clothes washed in Gibraltar.

"A gentleman having told me that he had reason to believe one of the Health Guards on board the '*Dygden*' had said that the captain had told him that all his crew had been attacked at the Havana by a fever called the '*Dengue Fever*.'

"He explained, on being questioned, that the captain had told him of the existence of such a fever in that city, which was very general, lasting four or five days, and then passing away without killing the patient, but that none of his crew had been attacked by it.

"I know not what new ground of attack is preparing, but it certainly is not abandoned, though at present the proof of introduction by the '*Dygden*' is, it would seem, rested on the declaration that fever issued out of the sugar boxes on their being opened here; whether any of them were so or not is more than

I can say, or if the Yellow Fever can be packed up and let out of a box of sugar. My opinion, founded on long experience, is, that the disease we are cursed with is not the Yellow Fever, but that which we had here in 1804, 1810, 1813, and 1814.

"Gibraltar, the 10th of November 1828.

(Signed)

"WM. SWEETLAND."

*Translation of the Bill of Health of the Ship "Dydden."*

"We, the members of the Board of Health, Don Antonio Gaston y Navarretti, Knight of the Royal Military Order of St. Hermenegildo, wearing the crosses of distinction of the Marine and Chilana, Captain of a frigate in the Royal Navy, and likewise of this Port, by his Majesty's nomination; Doctor Ciriaco de Arango, deputed chief magistrate by the most excellent Senate; and Doctor Don Lorenzo Hernandez, Physician, Honorary Consulter of the Royal Armies, and first Physician President of the Royal Medical Council of this always most faithful city of St. Christopher of the Havana, and ever loyal island of Cuba—

"Do hereby certify, that by the grace of God our Lord, at the sailing from this Port for that of Gibraltar, of the Swedish ship "Dydden," Captain Gerle, this city and its neighbouring towns are free from all plague or contagious epidemic disease, as likewise the said captain with the fifteen men of his crew, are in a state of perfect health, according to the muster by her roll, and in compliance with the last order, we give the present Bill of Health.\*

(Signed)

CIRIACO DE ARANGO,

ANTONIO GASTON,

DR. LORENZO HERNANDEZ."

Havana, May 9, 1828.

LETTER (G).

*Replies of several Medical Officers to some of the Queries sent out from the Army Medical Department in London, after the termination of the Epidemic of 1828 at Gibraltar.*

STAFF-SURGEON DOW, BREVET DEPUTY-INSPECTOR OF HOSPITALS.

"Do you consider this disease as originating in Gibraltar, or as an imported disease? State the facts on which your opinion is founded.

"I consider this disease as originating in Gibraltar. [Does not give the facts upon which this opinion is grounded.]

"In what respects does the late epidemic differ from the bilious remittent fever of the same place at certain seasons of the year?

"The principal difference between the late epidemic and the bilious remittent of the same place, appears from the remissions of the latter disease, and in the greater severity and rapidity with which the symptoms in the former run their progress, especially with reference to its fatal tendency.

"Do you consider the disease contagious, i. e. propagated by contact, and did the very latest cases appear to have been contagious?

"I know of no well-authenticated case of this disease being propagated by contact.

\* It is to be observed, that by reference to old Bills of Health, in the possession of the Captain of the Port of Gibraltar, whenever cases of Yellow Fever appeared casually at the Havannah, the circumstance had been invariably stated on the back, and the Members of the Board saw that on the Bill of Health of the "Dydden" there was no such note; so that this document proved not only that the Yellow Fever did not reign epidemically at the Havannah, but that no cases occurred sporadically when the "Dydden" lay there.

“Do you consider the disease epidemic, propagated by the general vitiation of the atmosphere of the Rock?”

“I consider the disease to have been propagated through the general vitiation of the atmosphere of the Rock, because the disease did not attack the inhabitants who sought refuge on board ship and on the Neutral Ground. The atmosphere of a sick room is not exempt from the general vitiation, at the same time I do not deny that in crowded, filthy, ill-ventilated apartments, persons are much more liable to be attacked with fever than they would be in clean, thoroughly-ventilated apartments, even should the latter contain sick people. I also consider the higher parts of a situation no security, provided the means of ventilation and of cleanliness are proportionably deficient.

“Had malaria or the effluvia from marshy places any, and what, influence in the production of this disease?”

“I do not think the effluvia from the marshy places of the coast, in the vicinity of the Rock, had any influence in the production of the late epidemic.

“What were the sequelæ of this disease; visceral obstructions, ague, &c.?”

“Hepatic derangement and general debility causing protracted convalescence, have been noticed as the most frequent sequelæ of this disease.

“Were other acute diseases observable during the epidemic,—particularly intermittent fever?”

“I did not observe any case of intermittent fever during the epidemic; and a few cases of dysentery and diarrhœa appear to have been admitted into the 94th Hospital during the early part of the epidemic.”

STAFF SURGEON DIX, DATED GIBRALTAR, 1829.

“What was the state of the public sewers, and was any marked difference observed in the cleanliness or filth of the streets, houses, &c.?—Report at large on this subject.

“The state of the public sewers has been in a state of progressive improvement for some years, and no cause of sickness appeared in them, which did not exist in former years; though I suppose that the sewers do in some measure tend to a general vitiation of the atmosphere, as some of these leading from the upper roads, are, I believe, very near the surface, and from the long absence of rain, at the period fever takes place, are nearly choked. Moreover, I have been told by the Surveyor of Revenue Works, that many of them are not carried out to low-water mark on the line wall. In the state of the streets or houses, I am not aware that any cause existed which was not present in the same, or in a worse degree in former years.”

On the subject of sporadies, Mr. Dix says—

“I only saw ten or twelve cases of the late epidemic; but those appeared to me to differ, in no respect, from bad sporadic cases of Yellow Fever I had witnessed here annually from 1820 to 1826.”

“Do you consider the disease epidemic, propagated by the general vitiation of the atmosphere of the Rock?”

“Yes, epidemic: and propagated by general vitiation of the air of the Rock; because any one, not having passed it, was almost sure to take it if they ventured to sleep in town.

“Had malaria, or the effluvia from marshy places, any, and what influence in the production of this disease?”

“Malaria, I suppose to have given origin to this disease.”

SURGEON M'LEOD, 42ND REGIMENT, DATED APRIL 16, 1829.

“Do you consider the disease contagious, i. e. propagated by contact, and did the very latest cases appear to have been contagious?”

“I never, in any instance, knew the disease to have been communicated from one person to another.”

ASSISTANT-SURGEON BULTEEL, 94TH REGIMENT.

“Were you ever in the West Indies or Gibraltar, and have you in either place or elsewhere witnessed a similar visitation of sickness?”

"I have never been in the West Indies, but at all the different stations on the western coast of Africa, and have witnessed a similar visitation of sickness.

*"Do you consider this disease as originating in Gibraltar, or as an imported disease? State the facts on which your opinion is founded."*

"Having no facts to elucidate from my own personal knowledge, relative to the origin of the disease, and not being able to discover any of its being contagious, I am naturally led to conclude that it is of local origin, from the great heat and drought, the former being greater than what I have experienced in Africa, and the lofty situation of the Rock impeding ventilation when an easterly wind prevails, [a fog or cloud] covering its summit and approaching to its base, causing febrile exhalations, which, on my arrival here, were prevalent and long continued: I evidenced the mitigation of the epidemic in November, and the visible improvement in the worst cases on the approach of a westerly wind, which was only for a short time, when an easterly re-appeared.

*"Do you consider the disease contagious, i. e. propagated by contact, and did the very latest cases appear to have been contagious?"*

"I do not consider the disease to be contagious; out of the number of orderlies, who were in constant attendance upon the sick, and necessarily subject to the most unpleasant effluvia, only one took the disease and recovered, which I attributed to bodily fatigue and watching, together with a recent fit of intoxication; and, on my arrival, a number of men affected with other diseases who were sleeping in the same wards with the fever cases, and attending upon the sick, escaped the disease, the former of whom I of course removed into a detached ward; and an orderly of the 94th Regiment, at the Naval Hospital, who it has been stated caught the disease, while in attendance on the sick on the 6th September last; from the man's own statement, he had been in No. 24 District, in company with a serjeant of the 12th Regiment, who also took the disease; therefore, from these strong examples, I form my opinion. Another very strong example is, that the officers used constantly to accompany me around the wards to visit the sick of their own companies, and also two of their brother officers who were seriously ill with the disease, and were in the habit of shaking hands with them; I can positively assert that not one of them took the disease.

*"Do you consider the disease epidemic, propagated by the general vitiation of the atmosphere of the Rock?"*

"I do consider the disease epidemic, from the men most invariably having been taken ill when on guard, or after having been in town, where the vitiation was much greater; but I know an instance of one man who was serving as cook to the Grenadier Company, who never was out of camp at Windmill Hill during the epidemic, but was at the canteen with a number of other men, when it was stated that there was a boy ill with the disease in the adjoining room. The man himself states, that he did not go near the patient; he, however, was taken ill after a fit of intoxication, being generally a man of sober habits. I can also state, from good authority, of a girl having been affected with the epidemic at the Signal Tower, who never had any communication with the town.\*"

ASSISTANT-SURGEON BROWNE, OF THE 23RD REGIMENT. DATED APRIL, 1829.

*"What was the state of the public sewers, and was any marked difference observed in the cleanliness or filth of the streets, houses, &c.? Report at large on this subject."*

"I was not in the garrison at the commencement of the late epidemic, and cannot say. But goes on to state some remarkable proofs from official authorities that the sewers were in a bad state.

*"Do you consider this disease as originating in Gibraltar, or as an imported disease? State the facts on which your opinion is founded."*

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\* This authority rendered quite null from investigations of Dr. Chervin, as related in one of his works.

“I consider the late epidemic fever, as originating in Gibraltar, for the following reasons; viz., 1st. Because I know of no proof of its importation, and because, when it has been imported into other places, even on a large scale, no epidemic has been produced in consequence. 2nd. From the non-communication of the disease by patients labouring under it, by convalescents from it, or by any articles used by the sick, when such patients, convalescents, or fomites, had been removed beyond the local source of infection, which in the late epidemic, was the case on the Neutral Ground, Windmill Hill, and Europa Flats; no person in any of these situations, having contracted the disease, from coming in contact with patients, convalescents, or their dirty clothing, &c. I am aware that a few individuals, amounting, perhaps, to 15 or 20, sickened in these situations, without having been known to enter the garrison; but, at the same time, it is an established fact that fevers of a highly aggravated nature, are contracted annually in the first of these localities, which furnished at least three-fourths of the total number of such cases; and, while numerous instances are to be found, where women, (soldiers’ wives) living in camp in these places, washed the dirty linen, &c., of fever patients, without injury, is it to be credited that a little black vomit on the skirt of a coat caused the disease? I may cite the exposure of the orderlies, and patients in the Naval Hospital for other diseases; in proof of this position, not one of whom contracted the epidemic fever, until it had previously appeared among the civilians, in the immediate vicinity, who had no intercourse whatever with the sick, and were in general extremely anxious to avoid coming near them. 3rd. From the disease being epidemic in autumn, or rather during the last six months of the year only, a season so notorious for the production of fevers from malaria, and also from its being like them, checked suddenly by a reduction of temperature, so inconsiderable as not to influence other diseases reputed contagious. 4th. From the peculiar severity of the disease as contracted in certain localities, showing thereby a difference in the concentration or virulence of the cause in these situations, a fact generally observed in the fevers from malaria, and so very different from the usual operation of contagion, especially, as these places are often far from being either the most crowded and filthy, or even the worst ventilated. In proof of what is here asserted, I may mention the highly aggravated nature of the cases admitted into the Military Hospital from North Flat Bastion, Southport, and Convent Guards, which were furnished at an early part of the epidemic, chiefly by the 43rd regiment, and also the very great mortality among the men employed in the cultivation of Rouvier’s and the adjoining garden in the south. 5thly. From Yellow Fever never appearing in an epidemic form, in any situation, where sporadic cases of the same disease do not occur; such cases demonstrating the existence of the local cause, though its operation may be limited by circumstances, unknown to us, influencing the production or propagation of malaria. That sporadic cases of Yellow Fever occur in this garrison, nearly every year, is a fact beyond dispute, for the difference of duration supposed by some to distinguish them from the epidemic disease, is a natural consequence of a cause less extended and probably less active, and after all it is a difference purely supposititious, as very many cases in the late visitation were equally protracted. The test of immunity from second attack, is also in favour of the identity of the disease, in the great majority of instances. It is said that sporadic cases of variola occur, without being followed by an epidemic, and so also may cases of Yellow Fever, but I know of no instance of such sporadic cases of variola, appearing annually in certain situations, differing only in number in different years; nor of epidemic variola, losing uniformly its contagious quality, when a subject labouring under it is removed from a certain locality. 6thly. From the frequency of relapses in the late epidemic, nearly one-fifth of the cases which occurred in the regiment (23rd) having suffered from a second attack, and one or two even from a third. This fact is out of all analogy with any known contagious disease, such as small-pox, measles, scarlet fever, plague, or even typhus, the contagion of which last is much more doubtful. 7thly. From the almost

total disappearance of every other acute disease from the garrison, during the prevalence of the late epidemic, an occurrence not to be observed in any epidemic of a contagious nature, such as small-pox, measles, &c.; these diseases prevailing often simultaneously, and without exerting any evident influence on the propagation of each other mutually; a proof that the epidemic Yellow Fever, proceeds from a local vitiation of the atmosphere, while the small-pox, &c., depend, in a measure, upon a specific contagion for their propagation. 8thly. From Yellow Fever being confined within certain parallels of latitude, and under certain altitudes within these parallels, and also for its attacking emigrants from northern climates, or healthy districts, with greater severity than the inhabitants of the places where it prevails, or persons acclimatés, peculiarities in which it coincides most strikingly with the more concentrated forms of the fevers of malaria. I need scarcely say, that the British troops in Lieria, (Portugal,) in 1827, suffered from remittents in a trifold ratio to the inhabitants of that place, and thousands of similar instances may be adduced. 9thly. From Yellow Fever attacking, in some epidemics, three individuals in the lower floor of a house or barrack, for one in the upper story. It was observed by assistant-surgeons Brown, of the 43rd, and Gillicie of the 12th regiment, that the orderlies and others, who slept in the lower wards of the Naval Hospital, during the late epidemic, were generally attacked sooner than those who slept in the upper wards, beside the sick. In Bosano's family at Rosia, every individual susceptible, eight in number, in the upper story escaped the disease; while every person in Belasco's family, living directly under them, who had not already passed the disease, was attacked. 10thly. From the failure of all attempts to communicate the disease by inoculation with blood, black vomit, saliva, &c., or by swallowing these fluids; and from the inutility of fumigation, expurgation, and every other "sanitary" measure whatever, excepting the removal of the inhabitants from the particular locality, in arresting the progress, or diminishing the mortality of the disease.

*"In what respects does the late epidemic differ from the bilious remittent fever of the same place, at certain seasons of the year?"*

"Bilious remittent fever, properly so called, is not a common disease in Gibraltar, and the cases of it occasionally seen in the hospital, are generally to be traced to the Neutral Ground; but the common continued autumnal fever of the garrison does not differ in any respect, from the majority of mild cases of the late epidemic. In the autumnal continued, instances are not uncommon in which death took place from the 2nd to the 7th day, and the cases generally either terminate in convalescence or in death, or become distinctly remittent or intermittent from the 7th to the 14th day; in point of duration, therefore, the difference between the fever of ordinary years and the late epidemic, is much more fanciful than real, in as far as my very limited knowledge of the subject enables me to judge. The peculiar appearance of the eyes, the nature and seat of the headache; and the want of remissions, are symptoms common to both, though more generally present, and better marked in the epidemic disease. The appearance of the yellow skin, black vomit, dark and black stools, and hemorrhages, are rare in ordinary years, but taking 1826 as an example, nearly thirty such cases are said to have occurred in the military and civil hospitals, exclusive of those of the ordnance, and 64th regiment, in the last of which several cases are said to have been treated. I do not affirm that black vomit presented itself in all these cases, but a few of them had that symptom. several had dark stools, and all of them yellow skin, while the number of fever cases in the returns of the 12th, 43rd, and 94th regiments quartered in the South, prove to demonstration, an unusual prevalence of that disease among them, as compared with the 42nd, quartered at Windmill Hill, or even with the 23rd, quartered in the town. I conceive that this extraordinary prevalence of a fever in the south district, the importation of which was never dreamt of, and when the symptoms of yellow skin and black vomit were frequent in the garrison, beyond proportion as compared with this disease in ordinary years, will form, like 1810, one of a series in the progression of fevers originating in

Gibraltar, of which the yellow malignant pestilential is the last and most aggravated. The only difference between the epidemic and the autumnal continued fevers of the garrison, are the epidemic prevalency;—the more frequent occurrence of yellowness, hemorrhages, black vomit, and dark or black stools;—the far greater proportional mortality; and the much less tendency of the former to terminate in a remittent or intermittent form after the fifth or seventh day. The great frequency of pulse, and heat of skin, are symptoms common to the autumnal continued, and the epidemic disease, but they are by no means peculiar to the latter, as the pulse not unfrequently beats from 45 to 90, especially in cases where delirium, or a degree of stupor, set in early in the disease, and the heat of skin is often moderate, and in the fatal cases, generally much reduced for several hours (sometimes 48) before death. The most uniform *post mortem* appearance, namely, change of colour in the liver, is found in both, as, also, the dark coloured spots round the cardia, so frequently supposed to be gangrene of the stomach. I have avoided contrasting the epidemic with bilious remittent, because the last disease is rare in the garrison, but it is necessary to observe, that the pale yellow colour of the eye and skin, (said to be peculiar to the epidemic,) vibices, hæmorrhages, the presence of black vomit in the stomach after death, and its ejection during life, have also been described in epidemics of a remittent type in Italy, Switzerland, and elsewhere in Europe, as also in the East Indies.

“*What are the diagnostic symptoms?*”

—“In answering the preceding question I have noticed the chief distinctions between the epidemic and common continued fevers of Gibraltar; and I need scarcely observe, that the supposition of the sporadic cases which appear annually in the garrison, being either cases of hepatitis or icterus, is not within the scope of credibility.

“*Do you consider the disease contagious, i. e., propagated by contact, and did the very latest cases appear to have been contagious?*”

“I know of no evidence of the disease having been propagated by contact; as the child sleeping in the same bed with the father or mother labouring under the epidemic often escaped it, and some individuals who had attended all the other members of a large family were equally fortunate. It was rarely that two soldiers in the 23rd Regiment were admitted from the same tent within a month, and an example is scarce to be found where this happened within a week, and in no instance, without the second person attacked having mounted a guard in the interim. Married men often passed the first night after their attacks in the same tent with their wives and families; and I know of no instance among the military where any bad effects followed this want of precaution. In short, no evident relation could be traced between the first introduction of sickness into a house and its after progress through the family, some weeks, and even two months, intervening in many instances between the first and second cases in the same house, so long as the disease was not general around; but as soon as it became frequent in the vicinity, all the other members of the family, hitherto healthy, often sickened in a few days. The instances are very numerous, on the contrary, where the individuals of a family sickened in rapid succession within the sphere of epidemic influence; but this is more peculiarly a consequence of a general vitiation of the atmosphere than of a contagious quality in a disease, and proves little for either side of the question. The very latest cases did not seem to vary from the rest by seeming to possess any contagious property.

“*Do you consider the disease infectious, i. e., propagated by the infected atmosphere of a sick room?*”

“It may be stated as a fact, that the atmosphere of a sick room does not communicate the disease with greater facility than the general atmosphere of the districts where the epidemic prevails; and, out of these districts, the disease is no longer epidemic, nor is it anywhere transmissible. This inference, in my opinion, is warranted from the persons who went to St. Roque and elsewhere, and sickened there, not having affected the health of their immediate attendants;

from those who sickened in the military and civil encampments, having, in no instance, propagated the disease; and from the orderlies in the Naval Hospital having remained unaffected until the inhabitants of the adjoining houses were already attacked with the epidemic. It is necessary to remark, that the attacks of the orderlies in the Ordnance Hospital, and of a nurse in the Civil Hospital, are said to be exceptions to what is here stated; but the facts with regard to the former, in so far as I know, are defective in many points absolutely essential to determine the question; and the nurse was in the habit of visiting her husband, a soldier of the 23rd Regiment, when he was on duty in the town, and was with him at South Port guard a short time before her attack; she also frequented a lodging-house in the lower part of 24 District, occupied by a serjeant's wife of the 12th Regiment.

*"Do you consider the disease epidemic, propagated by the general vitiation of the atmosphere of the Rock?"*

"I consider the disease to be produced by a vitiation of the atmosphere of the Rock, and that this malaria is more or less active in proportion to the distance from its source or to its accumulation and concentration in particular situations.

*"Had malaria, or the effluvia from marshy places, any and what influence in the production of this disease?"*

"There is no marsh, properly so called, within the territory of Gibraltar, nor even within a considerable distance of it; besides, the currents of air passing over the marshes of the adjoining country can seldom reach the garrison, as they must previously pass the Neutral Ground, where the wind blows for seven days out of eight from east to west, or *vice versâ*, so as to interrupt the progress or change the direction of the breezes, carrying with them marsh effluvia.

*"Were critical days observed in the course of this fever?"*

"The fifth day was more frequently fatal than any other, and the average duration of the disease was about seven days.

*"What were the sequelæ of this disease, visceral obstructions, ague, &c.?"*

"In several cases in the 23rd Regiment an obscure remittent form of fever supervened the epidemic attack, but its paroxysms did not always begin with chills, nor did they uniformly terminate in sweating, and it was as irregular in its accessions as in any other part of its course. I have seen jaundice in one case, hepatitis in two, and, in a few, dysenteric affections of more or less severity, follow the epidemic disease; and, in one subject, it appeared to develope phthisis, which proved most rapidly fatal; one or two cases, admitted towards the end of the epidemic, became distinctly remittent from an early period.

*"Were other acute diseases observable during the epidemic, particularly inter-mittent fever?"*

"Every other acute disease had almost disappeared; and intermittents and remittents, which were comparatively frequent in the corps since its return from Portugal up to the commencement of the epidemic, also ophthalmia, were no longer to be met with.

*"Were persons who had suffered once, or had had the Yellow Fever of the West Indies, liable to a second attack? state number of each."*

"I believe the immunity from second attack is very great; but, at the same time, I do not consider it so absolute as common report may seem to warrant. The only soldier in the regiment who asserted that he had passed the disease during an epidemic period in Barbados, and who had served eight years in the West Indies in another corps, died of an attack of the late epidemic. It would seem from the table in the Appendix\* that a severe attack of remittent gives a comparative immunity against Yellow Fever, but it is on so very limited a scale as, of itself, to warrant no conclusion. However, as few individuals who have

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\* Note from Dr. Browne's Table in the Appendix to his Report.

Proportion of fever cases to general strength, 142 in 530, or 1 in  $3\frac{1}{2}$ .

Proportion of epidemic cases after intermittent, 25 in 93, or 1 in  $3\frac{1}{2}$ .

Proportion of epidemic cases after remittent, 11 in 61, or 1 in  $5\frac{1}{11}$ .

resided any length of time in a warm climate have not suffered from remittent, and as such long residents enjoy an immunity independently of any previous attack of Yellow Fever, it may be a question yet to be determined how far residence simply, or how far having suffered from remittent, may contribute to this exemption.

*“What is the state of health in the adjacent parts of Spain and Barbary?”*

“I do not know that any epidemic disease prevailed in the adjacent parts of Spain or Barbary; but it was asserted, in November last, that some cases of fever of an aggravated form had appeared in Algeciras, and, it has since been said, that they were smugglers who communicated with the garrison. The latter statement is important, if true, as proving the non-contagious nature of the disease; while the former would be equally a proof of the influence of physical causes and of local origin.”

#### LETTER (H.)

##### *Catalina Fenic's Statement.*

Gibraltar, November 14, 1829.

“Catalina Fenic, widow of Felix Fenic (alias Mateo) has this day made a statement, in presence of the undersigned, to the following effect, and which, if required, she states her readiness to corroborate, on oath, before competent authority:—

“Declares herself to be the widow of Felix Fenic, who died in the autumn of last year: says that her husband was about 68 years of age, and of a broken constitution; that he had resided in Gibraltar about 33 or 34 years.

“States that, in 1804, she was the wife of a man named Salvador de Ortega, who was then the intimate friend of Felix Fenic, the person above alluded to, and to whom she was married in 1805, after the death of her first husband (Salvador de Ortega), who died of the epidemic fever in 1804.

“States, and is ready to make oath, that her second husband (Felix Fenic) had, in 1804, a severe attack of the epidemic fever of that year, and that she and her first husband (Ortega) visited him frequently; that, in fact, she herself attended him jointly with another woman, since dead; that, during his illness, his residence was in a small house at the bottom of the stairs, in front of the Civil Hospital, then called the Blue Barracks; that, during the epidemics of 1813 and 1814 her husband resided in Gibraltar without experiencing any indisposition from the fever of those years.

“States that, after the death of their two children (Salvador and Catalina Fenic), mentioned in the proceedings of the Board of Commissioners to have taken place about the middle of August, 1828, her husband became greatly distressed in mind; that, on a certain Thursday, the police came to their house to warn them, in common with others of the district, to remove into camp; that, on that day, her husband particularly complained of a large rupture which he had had for many years, and which occasionally troubled him, being worse than usual, and increased in bulk; that she had her husband brought to the Civil Hospital next day, when he was seen by Mr. Fraser and others, but not being a fever case he was sent back to his house: on the evening of the next day they were, like all others in her district, sent to camp on the Neutral Ground.

“In camp her husband was seen by Dr. Hennen and others, but was not ordered to the lazaretto, to which all fever patients were sent: he died in their tent on Monday morning; the tent was not ordered to be fumigated or washed by any one, and that she took no precaution to prevent contagion, as she was sure her husband did not die of the fever: declares that there was no vomiting, no yellowness of skin, or other usual signs of a bad fever. Says, that he complained greatly of a pain in his throat, and that it was a question among the medical men who saw him as to bleeding him, but that they said they had no lancets with them.”

"With reference to her husband having gone on board any ship in the bay at any time last summer, she most positively asserts that it was not so, as he never went out anywhere (he being an old infirm man) without letting her know; is sure he had not been in a boat for 10 years past.

"She is equally certain that her two children, Salvador and Catalina Fenic, were not on board any boat or ship, as stated in the proceedings of the Board of Commissioners, as they were always under their eye, and were not running about like other children, or ever in the habit of going on the water.

"States that, with respect to the boy Francisco Caffiero, neither she herself, nor any of her family, knew anything about him; and that what he stated to the Commissioners as to her boy Salvador (13 years of age) and Catalina (11 years of age) having gone on board of ship is a made-up falsehood.

"States, that her children had not any of the symptoms of the epidemic of Gibraltar, with which she had been familiar during her long residence; that the doctor (Lopez) observed some indigested portions of figs in what they passed by stool; that they had no vomiting; that their appearance after death was not changed like that of those persons whom she has seen die of the Yellow Fever, and that she cannot make herself believe that they died of that disease.

"Says that, during the illness of her children, five of their playmates (the children of her gossip Juaguina) constantly came to see them, but that none of these children were taken ill; that the whole of these children, however, passed the epidemic afterwards, when permission was given to some of the families to return to the expurgated habitations.

(Signed) HUGH FRASER, *Surgeon of the Civil Hospital.*  
A. BROWNE, *M.D., Assistant Surgeon 23rd R. W.F.*  
J. GILLKREST, *M.D., Surgeon 43rd Regiment."*

"I, the undersigned, do hereby certify that, on this 13th day of May, 1829, Catalina Fenic, in the foregoing statement named, appeared before me, and I then interpreted to her the whole of the said statement, which she confirmed in every particular in the fullest and most positive terms, and added that her late husband, for several years previous to his death, left off going at all into the bay, and that, being an old and infirm man, he gained his livelihood by purchasing tobacco and making it into cigars.

(Signed) ALEXANDER SHEA, *Notary Public, Gibraltar."*

#### LETTER (I.)

Of the Yellow Fever outbreak at Leghorn in 1804, which has been usually termed an epidemic, though confined to a few streets, an account was given by Dr. Palloni immediately on the cessation of the disease.

He concludes as follows:—

"I shall terminate this short essay with two brief observations. The first is, that if, on the one hand, the description which we have exactly given of the malady prevailing in this city, shows the greatest analogy with the typhus icterodes of Sauvages and Cullen, and consequently with the West Indian fever, it is, on the other hand, proper to confess, that either from local circumstances, or from difference of climate, or from a less diffusibility of the morbid atmosphere, its effects and propagation have been with us infinitely more mild and more limited than in any other part where an analogous disease has developed itself.

"The other observation, which may serve as an explanation to the preceding is, that the infection of this fever is of such a nature that the pure and renewed air decomposes the morbid agency at a small distance from the sick

person ; on the contrary, an air stagnant and full of animal exhalations becomes easily a vehicle for it. This is why it has more particularly manifested itself in the dirty and badly-ventilated streets of the city, and especially in the houses of the poor, among whom, besides the individual disposition necessary to the action of every contagion, the smallness of the chambers, the want of cleanliness, and the crowding of the inhabitants multiply the points of contact and facilitate infection. On the contrary, in clean and spacious streets, and in commodious and well-ventilated houses, the disease has either rarely manifested itself or been easily checked. Thus, we can confirm the remark of Currie, that even in hospitals well-situated and clean, the morbid infection has been confined to the individual attacked, without spreading to those suffering from diseases of a different kind, in this respect differing from other epidemic and contagious diseases. And, finally, for the same reason, the fever of which we are speaking, has for the most part raged in maritime cities without extending to the interior of the country ; and in those who left the districts where it prevailed unless they were already ill when they set out, no germ of the infection has developed itself, the infection seeming thus to have been corrected and destroyed by change of air and situation. In fact, if we except two or three streets of Leghorn, which the malady seemed as it were to have chosen for its abode, few other parts of the city were attacked by it, and, with some exceptions, it did not extend to the smallest distance from the city, so that numerous persons went, and much merchandize was conveyed, daily from the most infected districts to the surrounding country.

“ A healthy man, if brought into contact with those suffering from the disease, never communicated the infection to others, unless himself attacked by it. And, in fact, though greater precautionary measures were taken regarding those who were really attacked by the malady, and regarding the clothes and other things which immediately belonged to them, yet we have not perceived that the other healthy individuals of the same family, or the other furniture of the same house, ever became a vehicle of infection. Money, merchandize, and other matters were in continual circulation both within and without the city, yet the infection was not diffused thereby. I have besides observed that a certain habit gradually acquired of receiving the impressions of this miasma, easily took away its power of acting ; in fact of the many priests that daily visited those suffering from the disease one alone was attacked and died from it ; and no assistant in the hospitals experienced its effects, and only two or three of the other people so long near the sick took the disease.

“ If, therefore, in order to take this infection, there is needed (besides a natural disposition) the vicinity or the contact of some one suffering therefrom, or of the clothes used by him during his malady, and the circumstance of an air confined, stagnant, and full of animal exhalations ;—if a small amount of habit is sufficient to elude its force ;—if healthy persons brought into the proximity of the diseased, and the merchandize exposed to the free air never carried the infection to a distance from the city ;—if, finally, pure air, and ventilation destroy this morbid agency in the vicinity of its centre, or of the diseased person ;—who does not see the difference between the malady prevailing at Leghorn and all other pestilential contagions ? How much less its force and its diffusibility ! And how false the ideas, how ill-founded the fears conceived respecting it in districts not very far removed from this city ! But we cannot adduce any stronger or more convincing argument of the utility of pure fresh ventilated air than that furnished by the happy results obtained in the new temporary hospital of St. James. Situated at no great distance from the sea, but far from the insalubrious exhalations of the city, and thus obtaining a free ventilation of the purest air, of which its construction and the disposition of its parts enabled it to enjoy all the advantages, it is truly worthy of remark that scarcely was this hospital opened for the reception of patients, when not only the virulence and extension of the disease in the city commenced to decline, but many escaped from death who seemed pre-

vously about to become its victims. It is wonderful to see how those suffering from the malady are scarcely taken from their houses, languid, prostrated, and as it were conquered by disease, and placed in this new asylum, when the vital principle seems in a moment to recover its power: they revive; they confess that a sensation of comfort succeeds to that of languor and anguish; and the malady, assuming a milder appearance, ceases to resist medical art, and yields to the suitable curative method already determined; and very soon the patients pass into a state of convalescence which is neither long nor painful."

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LETTER (K.)

*Extract from "Some Account of the Fever which prevailed in the Garrison of St. Ann, Barbados, in 1847, 1848, and 1849, by Dr. JOHN DAVY, Inspector-General of Hospitals."* Edin. Med. and Surg. Journal, October 1849, page 277.

(The 66th, 72nd, 88th, and Artillery were the regiments which suffered, and out of a force of 1,200 men, 196 deaths occurred.)

P. 281.—"Relative to the origin of the endemic, it must, I believe, be confessed that there was the greatest obscurity. The weather at its commencement was unusually cool and pleasant; before its outbreak, and while it lasted, the seasons were nowise peculiar. Several circumstances at the time were pointed out as probable causes, such as an accumulation of rank vegetation on, with urinous exhalations from, a neglected hollow between the artillery and stone barrack, and to windward of the latter; the state of the Savannah, and of the adjoining ground, defectively drained, liable to be flooded after heavy rains, to retain water stagnant for a while, and, after a period of drought, to become parched and fissured; the state of the grave-yard, lying between the barracks and the shore, in unsuitable ground, either too rocky generally, or sandy and unduly crowded; and, lastly, the breaking up of a considerable extent of ground to some depth, and the levelling it for the improvement of the works in the neighbourhood of the barracks, where the disease first appeared. Whether any one of these circumstances, or whether all of them combined, had effect or not in the production of the disease, I cannot venture to affirm. That it was of local origin seems to me hardly doubtful, whatever the local cause may have been. The fact most conclusive in proof of this was the isolation of the disease. During the whole period it was confined to the garrison of St. Ann, and, during a considerable portion of the period, to the barracks and quarters situated on the lower ground, where the drainage was most defective, and the conjectured local causes most active.

"An opinion, I am informed, has been expressed that the disease was imported, and by Her Majesty's war-steamer 'Growler,' from the coast of Africa. That ship was employed in conveying liberated Africans to the West Indies. She left Sierra Leone on the 12th of November, 1847, and arrived at Trinidad on the 5th of December. During the voyage, 46 deaths occurred amongst the emigrants from chronic dysentery, and two deaths amongst the crew from fever. The emigrants were landed at Port of Spain. No fever was there introduced, although no precautions were taken that I have been able to learn to prevent unrestricted communication. She took her departure from Trinidad on the 8th of December; arrived at Barbados on the 10th; proceeded from thence on the 18th, and reached the Bermudas on the 24th. There, according to established usage, having come from the coast of Africa with fever on board, she was placed in quarantine; the crew landed and kept apart—the sick from the well, and the vessel thoroughly cleaned and fumigated. On examining the holds and bilges, they were found in a very offensive state, from accumulated vegetable matter, rice, chips, and shavings, in a decomposing state. 'The atmosphere in the carpenter's store-room would not support combustion.' The fever was not communicated at the Bermudas to a

family of five persons associated with the sick on shore; but two individuals from the shore, who were engaged on board in conducting the cleaning of the vessel, contracted it, as did also a certain number of the crew similarly employed, and only those so employed. The total number of cases of fever under treatment from the 'Growler' were 75, of which 3 only terminated in death, 72 having recovered. These particulars I have collected from an 'Extract from the Journal of Mr. Robert M'Crae, Surgeon of Her Majesty's ship "Growler,"' a copy of which interesting document is now before me, with which I have been favoured by the head of the medical department of the navy, Sir William Burnett. Mr. M'Crae, who had the best opportunities for arriving at the truth, in his journal expresses himself satisfied that the fever originated in the ship from the causes referred to, and that it was nowise contagious. The facts he adduces seem to bear him out fairly in these conclusions.\*

"But even if these conclusions of his be doubted, there is no proof that the Yellow Fever in Barbados was derived from the 'Growler.' It was rumoured that the two men of the 88th Regiment who were first attacked, and who died, had been on board. The result of careful inquiry is that this rumour was groundless—a mere conjecture. Even considering the time of arrival of the 'Growler' at Barbados, on the 10th December, at noon, and that the first two fatal cases in the 88th terminated on the 19th of that month, one after a treatment of three days in hospital, the other of five days, it seems improbable that the fever was contracted by a visit to the ship, were there proof, which was altogether wanting, that such a visit had been made. Moreover, the wives of the two men first attacked also experienced the disease, and according to my recollection, for I cannot find any note on the subject, as soon as, or a little before, their husbands; and they too fell victims to it. They occupied a low, ill-ventilated room of the iron barracks, intended, not for the accommodation of troops, but for use as a store-room. It was here that the disease first appeared, and scarcely a person escaped the fever who, from inconsiderate indulgence, had been permitted to be there. They were married men and their families, to whom any place apart is always acceptable. Further, it is worthy of remark that the inhabitants of Bridgetown, like the inhabitants of Port of Spain, in Trinidad, were not visited by fever after the arrival of the 'Growler,' many of whom, in communication with the crew in affording supplies, must, it may be inferred, have been more likely to have contracted it, had the disease been contagious, than the troops in garrison. Even the character of the fever amongst the crew of the 'Growler,' and of that in the 88th, had not such a close resemblance as to warrant their being considered identical in species. Yellowness of the skin is not mentioned as having appeared in any instance amongst the former. The majority of the ship cases appear to have been of no great severity, and without the symptoms denoting the malignant character, a mildness well marked by the large proportion of recoveries, 1 only in 25 proving fatal, whilst in the garrison 1 died in about every 5 attacked. It is true that in some of the 'Growler' cases there was irritability of stomach, and that, in one which terminated fatally, death was preceded by 'black vomit,' a solitary example, as reported by the surgeon. But irritability of stomach is not an unfrequent accompaniment of the common remittent fever of the West Indies, and 'black vomit' is occasionally witnessed in it, or in sporadic cases of fever, which may be of the nature of Yellow Fever, although solitary. The symptoms of the two, viz., of Yellow Fever and of remittent, excepting when the diseases are strongly marked, having many points in common, and consequently difficult to be distinguished.

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\* "When the holds of the 'Growler' were opened at Woolwich, after her return from the coast of Africa, two men who slept directly over the hatchway were seized with fever, possessing all the characteristics of Yellow Fever, and in the course of a few days they both had black vomit and died in the Marine Infirmary, where they had been taken at the commencement of the disease." *Bryson on Climate and Diseases of African Station*, p. 224.

"The same persons who suppose that the fever was imported, of course also suppose that it was contagious or infectious. Whether Yellow Fever is contagious or not seems to be admitted to be a difficult problem. The manner in which those who have specially considered the subject are divided in opinion respecting it, is sufficient proof of this difficulty. I feel it right candidly to confess that at the outbreak of the disease I belonged to the class of non-contagionists, and that what I witnessed during its progress confirmed me in the persuasion. The fact of its being confined for so many months to the low localities in which it first appeared; not spreading to the troops occupying the higher barracks, though not more than 1,600 feet distant; and not spreading to them in the instance of the 7th Fusileers, when the 88th were encamped within a few feet of the upper barracks, and for some time were sending into hospital fresh cases of the disease; the manner in which the inhabitants of the populous and crowded town adjoining, Bridgetown, in unrestricted communication with the garrison, remained exempt from it; the small number of cases of the disease which originated in the hospital, whether amongst the orderlies attending on the sick, or amongst the sick themselves—such who were admitted with other ailments—are circumstances, it seems to me, not reconcilable with the idea of the fever being of a contagious character. I have alluded to the small number of cases of fever that originated in the hospital. From a return now before me it appears that from the 26th February to the 30th September, 1848, in the 66th hospital, one orderly only attending the sick was attacked with fever, and that slightly, and only three patients admitted with other diseases.

"Some respectable medical authorities hold a doctrine, as it were, intermediate between that of contagion and of non-contagion, viz., that a disease not originally contagious may become so under peculiar circumstances. To prove or disprove this is necessarily difficult. It must be kept in mind that every endemic or epidemic disease has, as to the manner of its occurrence, after its first appearance, a good deal of the character of an infectious or contagious disease. Every disease of the former kind has, at some time or other, been held to belong to the latter. Those who advocate this expediency doctrine might adduce in support of it the circumstance that, though the 7th Royal Fusileers, in communication with the 88th, escaped the fever, and though the 72nd Highlanders, for some months similarly situated, also escaped it, yet ultimately the disease appeared amongst them, and after its first appearance spread rapidly. In reply, it may be remarked that, considering how near the regiments were stationed in the same garrison, living in the same manner, performing the same duties, and often in common, and exposed, therefore, partially to the same causes, it is more surprising that the 72nd so long remained exempt, than that it was at length attacked.

"The history of Yellow Fever in the West Indies, from the early period when, by the contagionists of that time, it was called the 'Mal de Siam,' to the later period when it was named the 'Boulam Fever,' seems to me to afford corroborative evidence that in every instance it has been of local origin, and neither contagious at its commencement nor in its progress. From all the information I have been able to collect, Yellow Fever is not the prevailing fever of the western coast of Africa; it is doubtful whether it ever occurs there. Of this we are certain, that it rarely in the West Indies attacks individuals of the African race. We are also certain that those situations most exposed to infection or contagion from without, such as ports communicating most freely with other ports and marts of commerce, are not more frequently visited by Yellow Fever than spots where there are troops in a manner isolated. I think, too, it must be admitted that in no instance—not a single one—not even in that specially called the Boulam fever—has there been fair and satisfactory proof afforded that the disease was imported, and not of local origin."

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## LETTER (L.)

*Some Remarks on Yellow Fever from the pen of the late Dr. W. Fergusson, Inspector-General of Hospitals.*

"In regard to the contagion of Yellow Fever, all those best qualified to judge, that is to say, those who have spent their lives and devoted their services in the countries between the tropics, where it is so often present, are of the same opinion. I do not pretend to say that, amidst the heterogenous mixture of which our profession is composed, I have not heard some profess, and even subscribe, to a different belief; but I can declare that, I never knew a single instance of any one, provided he had had experience of the disease, acting as if he believed in its being contagious, or taking any of the precautions which the instinct of fear, or the smallest degree of common prudence, would, in that case, have dictated: he was contented with voting himself contagion-proof, and never scrupled to inhale the breath, or repose within the atmosphere, of the dying patient. \* \* \* I presume it will not be denied that those who served during the war in Saint Domingo, where so many British troops perished, must have had some experience of Yellow Fever. I remained there till the last, and saw the work of destruction completed. At first, every new comer, whether medical or otherwise, had his fears, or, I should rather say, had the firmest belief in contagion; but with none did that prejudice ever remain beyond the year: it vanished infallibly, as soon as he saw and had had experience of the disease; and I can declare that, during the latter years of our sojourn there, with hundreds of cases daily before our eyes, mixed in every conceivable way with the surgical, the convalescent, and the healthy, I never even heard the idea started, nor do I recollect a single precaution, advice, or observation, that acknowledged the existence of contagion ever being directed to the medical staff of the army from any quarter. I appeal to the writings of Dr. McLean, the evidence of Mr. Weir, Dr. Jackson, Drs. Theodore Gordon, Borland, Inspector Warren, and all the medical officers who served there to bear me out in this assertion. I appeal to the evidence of every medical officer now serving in the West Indies, that has ever had experience of the disease (for there may very probably be found contagionists among those who never saw it), to say whether, in their lives, they ever met with a case of Yellow Fever that could, with greater feasibility, be traced to a personal communication with a subject labouring under the disease than to the ordinary natural causes from which it has been proved to originate. \* \* \*

It would have done the same to the writers on the fevers of Spain if they had not dismissed all their doubts, and set inquiry to rest, by attributing the whole to imported contagion. But had they inquired instead of peremptorily deciding, they might have found that, during the epidemic visitation of these fevers, not only do towns and districts escape altogether, as we see at present in the West Indies, but that different parts of the same town are differently affected; and so limited often is their influence, that one story of a house and one section of a ship will be strongly affected by it, while all other parts of the same tenements remain healthy; and then their wonder at what has been called the desultory progress of the fever during the short course of an European autumn need not have been so great as to have been inexplicable through any source but that to which they referred it. At Barbados, our hospitals, of late, had been in a regular course of importation of the Yellow Fever from the navy; but not even inoculation has been able to produce the disease upon any member of the hospital corps, by whom I may truly say that *the sick had been* received with open arms, for the antisocial doctrines of ideal contagions are not preached amongst us here to the prejudice of duty and humanity. \* \* \*

The physician, in civil life, however superior he may be in learning and talents, as he sees only insulated cases, cannot have the same advantages, and must be much more liable to form erroneous conclusions. He may see, for instance, during the autumnal season of Europe, the inhabitants of a humid, unwholesome

house fall ill with dysentery one after the other, according to the degree of predisposition or stamina of resistance in the respective individuals, and thence conclude, with much *apparent* justice, that dysentery is a contagious disease; but take him to the well-regulated encampment, and show him its hospitals, what every experienced medical man has seen,—a thousand dysenteric patients attended by a hundred servants without affecting one of the attendants, he certainly would for ever be cured of the delusion.\* \* \* Unfortunately these opportunities of observation are not always sought for; and as experience of Yellow Fever in civil life is often trifling, limited to particular seasons and circumstances of exposure in the subject, he may, though difficult to experience, believe in his doctrine of contagion to the end, and do his utmost to alarm the public: but the military medical officer who has once seen the disease in the gross, as it affects newly-arrived troops here, never can be deceived in respect to its non-contagious character (if his mind be capable of distinguishing truth from error), and, in fact, never is in these climates.”

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LETTER (M.)

*Among the medical officers of experience in the West Indies from whom I have requested an opinion in reference to the contagion or non-contagion of Yellow Fever, is Staff-Surgeon Melvin, who served, I believe, no less than 20 years on that station.*

*In a letter written to a friend, Mr. Melvin says:—*

“MY DEAR DOCTOR,

“IN my last letter, very likely I did not express myself so clearly as I should have done about the Yellow Fever which occurred when I was last in Demerara.

“During the time I was serving there last I met with two severe visitations of it among the white troops in the garrison. During each occurrence of it most of the orderlies and men who came into hospital with other trifling complaints were attacked with it, and many of them died.

“I put a stop to it the first time this scourge of the West Indies made its appearance. I had the most of the white troops removed from the colony.

“When it made its appearance a second time, in a fresh corps, in 1842, the cases of true Yellow Fever were numerous and of the worst description—and during this visitation every non-commissioned officer who was employed about the hospital was attacked with the disease, as also the orderlies, (two of the former and three of the latter died)—I never had the most distant idea that contagion was the cause of these attacks among the hospital servants, nor with some of the patients who were attacked with Yellow Fever when in hospital under treatment for slight complaints; but that the cause was external, and from the very insalubrious situation of the hospital.

“To the windward of this hospital there was a large tract of ground which had been recently reclaimed by deposits from the sea. This ground was covered by the tides, and when they were out exposed to the hot burning sun, which could not do otherwise than produce plenty of exhalations from the muddy surface; and, besides this, on all sides, and very close to the hospital, there were filthy trenches, and the ground for some way all round the hospital was very badly drained, in hot weather cracking into large deep fissures, which during the rainy season were filled with water. I may state that the newly-formed ground was only a few yards distant from the hospital, and when in this country before, I have seen vessels of considerable size sailing over it.

“Thinking as I did about the origin and cause of this fever attacking the hospital servants and patients under treatment in it, I determined to try the experiment of removing the whole of the sick of the white troops from the regular hospital to a building in Kingston, nearly half a mile distant, and

which in former days was the hospital. I therefore removed all the sick. Some of the cases of Yellow Fever were very bad; one of them at the time was bringing up large quantities of black vomit, and had much hæmorrhage from the gums and nose, with an exceedingly cadaverous and disagreeable smell emanating from his body; in fact, he in every way appeared to be quite hopeless. However, to my delight, this case, as well as every other, perfectly recovered, which most assuredly would not have been the case had not the change of situation been made. And to my further delight and comfort I had not a single servant attacked with Yellow Fever, nor did I lose a single case from it while I occupied this old hospital; but I had cases of regular Yellow Fever among some of the families in the barracks. During the period I have been speaking of I attended the sick of the white troops entirely myself.

"With regard to the ground about this old hospital which I had the sick taken to, it was well drained, and, above all, it was out of the influence of the new marshy and muddy ground so very close to the new regular hospital.

"I cannot think how any one can be a contagionist as regards Yellow Fever."

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*Copy of a Letter from Staff-Surgeon Dr. Spence,\* formerly Assistant Surgeon to the 52nd Regiment, while serving in Barbados in 1838-9, dated Bishop-Wearmouth, 18th November, 1846.*

"MY DEAR SIR,

"KNOWING your desire for information relative to the cause of 'fever,' I have great pleasure in communicating to you a few observations upon an epidemic form of Yellow Fever as it affected the 52nd Light Infantry at Barbados in the latter part of the years 1838 and 1839.

"You are aware that the 52nd embarked at Gibraltar in Her Majesty's ship 'Hereules,' commanded by Captain Toup Nicholas, about the 13th October, 1838, in the most perfect state of health, and also that fever was not more than commonly prevalent at that time in the garrison; but on the day previous to embarkation the troops were exposed to a deluge of rain whilst at field exercise for the inspection of His Royal Highness Prince George of Cambridge, in consequence of which one man was subjected to an attack of gastro-enteritic fever, of which he died; but I pledge you my honour without one symptom of Yellow Fever, as I afterwards observed it. I cannot at this time get at my notes, and therefore am unable to state with precision how many days this death occurred before our arrival at Barbados, but think it must have been from 10 to 14; however, be that as it may, the regiment disembarked on the 6th November, without a medical case.

"On the day of arrival we occupied barracks together with the 36th Regiment, who the next day embarked in the 'Hereules.' Major Cross (having resided in the officers' quarters, afterwards tenanted by Mr. Winterbottom,) actually going on board with fever upon him, died in three days with 'black vomit,' whilst the vessel was in harbour, and his corpse was brought on shore for interment.

"On the 10th November, Lieutenant Gough, of the 52nd, first complained of indisposition, of which he died in three days; and between this period and the 31st December, out of 36 individuals, namely, officers, their servants, non-commissioned officers, and soldiers connected with the orderly-room and quartermaster's store, all of whom were occupied or residing in the building appropriated as quarters, and mess-room for the officers (out of which Major Cross had gone), 28 were attacked with fever, and 10 died, whilst of the whole regiment residing in the soldiers' barrack, so short a distance as 50 paces from the fatal spot, only 30 cases occurred, and none died.

"An investigation was instituted at the time relative to the cause of this

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\* One of the members of the Medical Board lately assembled in London to investigate the subject of Yellow Fever.—(See Appendix IV., p. 318).

fever, particularly having reference to the concentration of the pestilential essence in the officers' quarters, but after the greatest care nothing could be detected in the slightest degree calculated to throw light on the subject; but it was recommended that the quarters should be evacuated forthwith, and forthwith the fever stopped, for after this measure not one case occurred. Captain Vigors was in the height of fever at the time of the move, and was conveyed to a room in the house temporarily hired as a mess-house and quarters for officers, and although four officers, who arrived from England in the latter part of December, had free intercourse with him, and were constantly in the new mess-house, they were not attacked, because they scrupulously avoided the infected locality.

"Thus far, the cause of the malady was involved in the utmost obscurity, but at the time of writing this letter I entertain the fullest conviction that the 'malaria' occasioning such destruction was engendered either in the fresh-water swamp, occasioned by the heavy rain falling on low ground between the back of the hospital and the officers' barracks, or by emanations from a swamp containing much vegetable matter, and communicating with the sea, about a mile to the windward of the regimental hospital.

"The doctrine of the contagious nature of fever is in my mind so erroneous that I should not dwell thereon were it not with the view of enabling you to refute, so far as my feeble efforts can, the opinions entertained in reference to the origin of the fever on this particular occasion. In the first place, if the principle of contagion existed in the 'Hercules' (which I cannot admit, though I believe such to have been stated by Captain Nicholas), whether is it more reasonable to believe that it was brought on board by the soldier of the 52nd, who died without any characteristic sign of Yellow Fever, or by Major Cross, who died with a 'black vomit' three days after he embarked, and had a rigor the night before he left the garrison? Captain Nicholas, however, seems to think otherwise, and blames the unfortunate soldier of the 52nd for an alarming fever which affected some of the officers of his ship whilst conveying the 36th Regiment from Barbados to Halifax, but he omitted to state whether any were attacked but those who had dined with the officers of the 52nd at Barbados, and thus been exposed to the cause of fever existing in the locality of the mess-room, which question you will admit as one necessary to be decided before giving adherence to his views. Whilst on this subject, it may be well to attract attention to the fact that whilst the 52nd Regiment suffered so much, none of the other troops in garrison were in the least affected, which I think must have been the case had it been a contagious disease, brought by them from Gibraltar, for there was no cordon placed around the barrack, nay, indeed, all the sick soldiers were treated under the same roof as the soldiers of the 69th, and, to the best of my recollection, that regiment had but one case, and that occurred in an old soldier long seasoned in the country, who was lent by Colonel Monins to the officer suddenly called upon to perform the duties of paymaster, upon the death of Mr. Winterbottom. This poor man wrote in Captain French's room one day, and was dead three days afterwards. Now Captain French never had fever.

"On the 1st November, 1839, cases of this peculiar form of fever again appeared in the 52nd, taking a progressive course directly with the wind from the hospital (the serjeant and orderlies of which were first attacked), in a straight line across a swamp to the barracks. On this occasion the officers' quarters yielded three fatal cases only, and the disease appeared pretty generally through the soldiers' barrack, and in the huts occupied by the non-commissioned officers and married people. Thus, although the influence of the cause seemed to travel in a more extensive vein than in the previous year, yet the pestilence was almost entirely again confined to the quarters occupied by this regiment, as only three fatal cases occurred amongst all the other troops in the garrison. Therefore, at the recommendation of Mr. Draper, Inspector-General of Hospitals, the 52nd were moved to an encampment about a quarter of a mile to windward of the swamp before mentioned, instead of being to

leeward; and no case was admitted for a fortnight after the move, and, indeed, only three fatal cases occurred subsequently, and the individuals had been employed at the barrack.

“ I am, &c.,

“ THOMAS SPENCE, M.D.”

#### LETTER (N.)

*The following is an extract from a pamphlet on the Yellow Fever at Galveston (Texas) in 1839, by Dr. Ashbel Smith.*

This gentleman had studied in Paris, and was some 10 years ago employed as envoy from Texas to the British Government, he says:—

“ The *exclusive* confinement of the disease to persons much exposed in the infected district, the frequent removal of the sick from this district to the healthy parts of the city, without communicating the disease to the attendants or any other persons whatsoever, strongly confirm the opinion of its non-contagiousness.

“ The local causes are the decomposition of *abundant* animal and vegetable matters, going on under and around the houses on the ‘ Strand,’ and the exhalations from the extensive adjacent marsh and quagmire, exposed to an ardent sun, whose thermometrical range in the coolest shade for several hours daily, has been from 84 to 89 degrees of Fahrenheit.

“ The sick have been in numerous instances removed from the infected district to the healthy sections of the city, and in no case, as far as I can learn on careful inquiry, have the attendants or inmates of the houses contracted the disease. On the contrary, the city, with the exception of the infected quarter, has enjoyed its unsurpassed general healthfulness.

“ I have made several *post mortem* examinations, handling every organ without squeamishness, immersing my hands freely in the black vomit and other fluids, smelling and viewing them closely; I have repeatedly tasted black vomit when fresh ejected from the stomachs of the living; I am not aware of ever having experienced further inconvenience or effect than fatigue.

“ Of the physicians who practised in this disease all escaped an attack except two; these two *resided* in the *infected* district; the others lived without it.

“ After a careful observation of the history of the epidemic, no fact has come to light which would show that the disease is contagious, that it is communicable from a person labouring under it to one in health, but that it is contracted only by exposure in the infected district.

“ The subject of contagion is one of great importance. A belief in the contagiousness of this disease would deprive the sick of the most necessary attentions; non-contagion destroys many of the horrors of an epidemic, as a removal only a short distance places us beyond its influence.”

#### LETTER (O.)

*The following is a Resumé of the very valuable Manifesto of the 15 Physicians present at the Yellow Fever epidemic of 1821 at Barcelona.*

“ From all that has been stated it results—

“ 1. That the fever which prevailed in Barcelona in 1821 was *indigenous*.

“ 2. That it was epidemic.

“ 3. That it was *not* contagious.

“ 4. That the sanitary measures adopted by the Government were precarious, wholly useless, and even prejudicial, if we except that of *emigration*.

"5. That if, instead of remaining in a shameful inaction, expecting to knock on the head an invisible and imaginary contagion, unknown in its essence, and impossible to be demonstrated, all the means calculated to remove the local causes, were employed with constancy and energy, it might be hoped that the disease would not re-appear; that this beautiful capital would recover the degree of salubrity which it was formerly wont to enjoy; and that commerce and industry, in a word, that prosperity extending not only to all Catalonia, but to the whole of Spain, and even to the most distant nations, would revive.

"*Barcelona, February 21, 1822.*"

LASSIS (SIMON), D.M., of Paris. (a)  
 ROCHOUX (J. A.), D.M., of Paris. (b)  
 FRANCISCO FIGUILLE, M.D., of Barcelona. (c)  
 FRANCISCO SALVA, M.D., of Barcelona. (d)  
 MANUAL DURAN, M.D., of Barcelona. (e)  
 JUAN LOPEZ, M.D., of Barcelona. (f)  
 SALVADOR CAMPMANY, M.D., of Barcelona. (g)  
 IGNACIO PORTA, M.D., of Barcelona. (h)  
 JOSE CALVERAS, M.D., of Barcelona. (i)  
 ANTONIO MAYNER, M.D., of Barcelona. (h)  
 RAYMUNDO DURAN, M.D., of Barcelona. (l)  
 BUENAVENTURA SALMO, M.D., of Barcelona. (m)  
 JOHN LEYMERIE, M.D., Citizen of the United States. (n)  
 THOMAS O'HALLORAN, M.D., British Army. (o)  
 CHARLES MACLEAN, M.D., of London.

"N.B.—In order to obviate misconception, I think it right here to observe, that the first edition of the Manifesto, published at Barcelona, had only *thirteen* signatures, Drs. Leymerie and O'Halloran having left that city before it appeared. Their names were, however, added on the publication of the Madrid edition, augmenting the signatures to *fifteen*, being the original number of our spontaneous association."

(a) "Formerly Physician to the French Armies, and Physician-in-Chief of the Hospital of Nemours; author of "*Recherches sur les veritables causes de Maladies epidemiques, appellées Typhus,*" &c. Paris, 1819.

(b) "Member of the Medical Commission sent by the French Government to Catalonia; formerly Physician *en second* to the Military Hospital of Fort Royal, Martinique; author of "*Recherches sur la Fievre Jaune, et Preuves de sa non contagion dans les Antilles.*" Paris, 1822.

(c) "Vice-President of the Sub-delegation of Medicine; Clinical Professor; member of various learned bodies; an eminent practitioner, who, after having distinguished himself as a writer *in favour* of pestilential contagion, had the magnanimity to avow his conviction of the truth of the opposite opinion.

(d) "Honorary Physician to the King; Clinical Professor; Dean of the Faculty of Medicine of Barcelona; member of various academies. This venerable physician has been justly celebrated by Townsend for his courage and humanity in resisting, happily with success, a despotic and barbarous mandate of the Court, directing the exclusive administration of a nostrum of the King's physician, during a pestilence at Barcelona.

(e) "Member of the Academy of Practical Medicine; a convert from conviction at an advanced period of life.

(f) "Member of the Superior Junta of Sanidad of Catalonia; a man of great experience, modesty, and discernment; a convert from conviction.

(g) "Member of the Academy; Physician to the Military Hospital; the first to volunteer his services to attend the patients in the foul lazaretto. At the commencement so thorough a believer in contagion, as to have worn an oil-cloth dress.

(h) "Member of the Academy of Practical Medicine.

(i) "Member of the Subdelegation of Medicine.

(k) "A respectable practitioner, who occasionally exercised his talent in satirising the "sanitary" proceedings of 1821.

(l) "Physician to the General Hospital; wrote occasionally against the doctrine

of pestilential contagion in the journals; had himself the fever, in which he was attended by Dr. O'Halloran and myself, in consultation with ten or a dozen of our Barcelona colleagues. Extreme unction had been administered, but happily he recovered, to the great satisfaction of all his professional brethren, as well as to the benefit of our joint manifesto.

(m) "Physician to the General Hospital; had double labour during the latter part of the epidemic on account of the illness of his colleague, which he performed with much cheerfulness. Animated with a laudable anxiety to give information respecting the affairs of his department.

(n) "Formerly Physician in chief to the Hospital of Santiago in Paris, member of various learned societies, and attached to the American legation at Madrid.

(o) "Member of the medical academies of Madrid and Barcelona; author of two works on the epidemics of Spain; a volunteer, with permission of his Royal Highness the Duke of York, to investigate the fever of Barcelona, and to succour its suffering inhabitants."—Maclean's *Evils of Quarantine Laws*.

#### LETTER (P.)

*For a full Account of the Appearance of Yellow Fever at St. Andero in 1813, see the Thesis of Dr. Bone's Son, Dr. Hugh Bone, of the medical staff. (Edinburgh, 1846), p. 19.*

"In the autumn of 1813, being the close of the Peninsular War, sporadic cases of Yellow Fever appeared in the depôt barrack in St. Andero in Spain, and after the Christmas holidays became numerous. The barrack was inspected by Dr. Erly and by Dr. Bone, and was supposed, from the foul state of the sewers and privies, to be unhealthy. The troops were removed to a healthy situation, and soon became healthy. But the removal of the troops attracted the notice of the Spanish authorities; their board of health inspected the British hospitals, and pronounced the disease to be Yellow Fever, and contagious. The British medical officers were unanimously of opinion that the disease was not contagious; but the patients who were yellow were selected and put in quarantine in moveable hospitals in a healthy situation. Dr. Bone was in charge of the quarantine hospitals. Fifty of the patients were yellow, and eleven of them died, and were all carefully dissected by him or by his assistants, yet none of them caught Yellow Fever, nor any of the other patients in the quarantine hospital, nor any of the hospital servants or washerwomen, nor any of the patients or servants in the hospital from which the yellow patients were taken. He calculated that 700 persons had been exposed to the influence of the disease, yet none caught Yellow Fever. The cordon of troops did their duty, the British strictly, the Spanish with ferocity, but could not prevent all intercourse with the quarantine hospital.

"The president of the board of health had been a professor in one of the Spanish universities, and was learned, and, for a Spaniard, liberal and not bigoted. Having repeatedly and minutely examined the Yellow Fever patients in the quarantine hospital, he changed his opinion, and declared the disease to be non-contagious or little contagious, and in many cases only jaundice. The junta fined him for altering his opinion; he carried on a paper war with them; the argument was in his favour, but the power was against him, and they levied the fine; but the British officers in St. Andero, thinking it unjust that the professor should be fined for declaring the truth, raised the sum by subscription, and presented it to the professor, with a kind and well-penned letter by Dr. Erly, and the professor was compensated for the persecution of his bigoted countrymen."

LETTER (Q).

Dr. John Wilson, R.N., at page 142 of his book on West Indian Fever, referring to the case of the "Iphigenia," at Port Royal, Jamaica, on board of which Yellow Fever had appeared in February 1820, furnishes a letter from Dr. Bancroft, Deputy-Inspector of Hospitals at that island, addressed to Rear-Admiral Sir Home Popham, in which we have the following paragraph:—

"All we have been able to discover in its history and progress tends to convince us that the fever in the 'Iphigenia' has neither originated in, or been propagated by, contagion."

At page 144, Dr. Wilson furnishes a copy of an official document from Dr. Adolphus, Deputy-Inspector of Hospitals at Jamaica, 4th February, 1820. After stating that he had made a most minute inspection of the "Iphigenia," Dr. Adolphus says:—

"From the observations my experience of the diseases of this climate has enabled me to make, it appears that at least nine-tenths of European subjects, whether in civil or military life, are not only liable to, but are actually seized with fever within the first 12 or 15 months after their arrival in a tropical climate; that when the disease has once shown itself among a considerable body of men (as in the army and navy) it is sure to spread with unerring certainty to a greater extent, and under a more aggravated form, than when persons in civil life are the subjects of its attacks; not from any contagious properties, which I distinctly and wholly deny, but occasioned by the dreadful presentiments which the men invariably encourage under such circumstances by that terror of disease which is sure to predispose the constitution to febrile attacks, upon the slightest deviation from the established rules for the preservation of health, amongst persons unassimilated to the climate.

"When it is considered, therefore, that the crew of the 'Iphigenia' had never undergone what is emphatically called 'a seasoning,' in its terrific form it is not to be wondered at that sickness should at length make its appearance among them.

"With regard to the nature of the fever, I consider it, what time has proved it to be, the indigenous offspring of this climate; and its degree of mildness and severity to depend on the circumstances of season, of locality, and constitutional temperament; and, lastly, I consider the fever to have been produced by increased temperature and other atmospheric causes.

"J. ADOLPHUS, M.D.,  
"Deputy-Inspector of Hospitals."

Dr. Wilson next gives, page 145, a letter from Surgeon Macnamara, of the Naval Hospital, Port Royal, Jamaica, dated 7th February, 1820:—

"SIR,  
"Naval Hospital, Port Royal,  
"February 7, 1820.

"On the 14th of last month, when I had the honour of waiting upon you on board of His Majesty's ship 'Sybille,' the number of patients in this hospital amounted to only 17, the greater part of whom were convalescent. However, it is with infinite regret I find it my duty to state for your information that on the evening of that day two petty officers were received from His Majesty's ship 'Iphigenia,' labouring under fever, the symptoms of which were of so marked and unequivocal a nature as to induce me to represent to Captain Parker the absolute necessity of his proceeding to sea, to avert, if possible, the extension of the disease, which seldom fails of communicating its baneful influence to the whole of any ship's company where it has once displayed itself."

[He then states that, in consequence of his recommendation, the ship put to

sea, but returned at the end of four days, the disease having continued to prevail on board.]

"Immediately on her arrival, 13 cases of fever were sent to the hospital."

[He then proceeds to state the number of daily admissions and deaths up to the 6th of February, the total number of admissions from the ship being 121, 30 of which proved fatal, and continues:—]

"Notwithstanding every inquiry which I have been able to make in endeavouring to ascertain the remote cause of this destructive malady, it still continues to elude my research, nor have other medical gentlemen of great talent and experience been more successful in theirs.

"Indeed, sir, it is difficult to account for the generation of disease in a ship so well regulated, and in such a state of high discipline, as the '*Iphigenia*;' and I am most positively and decidedly of opinion that the disease, which has already committed such ravages on board that ship, is to be solely attributed to a particularly vitiated state of the atmosphere, the influence of which has been experienced along the whole of the American coast, from the northern bank of the Orinooko to Boston in New England, and in the adjacent islands.

"With respect to the local management on board the '*Iphigenia*' since the appearance of this disease, every precaution which human foresight and experience of the past could devise has been had recourse to, to prevent, if possible, the extension of so insidious an enemy. Her hold has been cleared, and fires kept constantly burning therein, and I can with confidence assert that her timbers are at this moment as dry as the head of her mainmast.

"J. MACNAMARA,

"*Surgeon of the Naval Hospital.*"

"To ————.

(Name of superior officer not given with this letter.)

At page 152 Dr. Wilson enters into his views as to the cause of Yellow Fever on board ship, which he attributes to a particular decomposition of the timbers, spars, &c., on board. This part of the subject, however, involving somewhat of theory, cannot here be entered upon with advantage.

#### LETTER (R).

On the reappearance of fever at *Boa Vista* in 1846 the Lords of the Admiralty ordered Dr. King, R.N., I believe one of the most experienced in Yellow Fever of the Naval Medical Officers, to proceed to that island for the purpose of affording assistance, and also to investigate any circumstances regarding the outbreak of fever in the preceding year as connected with the visit of the "*Eclair*." This gentleman, in his report, printed by order of the House of Commons, 10th March, 1848, says, page 1, "I venture to presume that I have succeeded in establishing arguments in support of my own views, deduced from indubitable facts, which remain altogether unnoticed in the report of Dr. M'William."

The "instructions" to both Dr. M'William and Dr. King were, I understand, precisely similar, but it does not appear that the replies from each were given categorically. The latter gentleman is most circumstantial in his details, as, for instance, where he states (page 9 of his report):—"That the common air which was inhaled by every living thing on the island was in an epidemic condition in the months of October, November, and December, in both years, is sufficiently demonstrated by the simultaneous occurrence of universal sickness and great mortality among the cattle (including cows, horses, mules, donkeys, and goats), at the very time that fever was raging among the inhabitants. And further, there was this remarkable coincidence, that after an

interval of some months, and the disappearance of disease both in man and beast, the same fever broke out again in the towns and villages about the rainy season in the following year, and was again accompanied by the same murrain among the cattle, which in the two seasons proved fatal to two-thirds of the whole stock of the island."

He states that, though not having personal opportunities of observing, he ascertained to his satisfaction that the disease which prevailed at Boa Vista was essentially the same as that which prevailed in the "Eclair:" that he examined at Boa Vista all the survivors said to have had connection with the "Eclair," as labourers, washerwomen, and soldiers; this was carefully done and their statements drawn up in the presence of the señor Baptista, and John Jamieson, the interpreter, who was the consul's storckeeper. He states, that the labourers attacked had not been exposed to contagion had such a thing existed; and that the fever was not communicated to the persons in the house into which the sick British officers and their servants had been received.

Dr. Almeida of Boa Vista, considered the disease alluded to as an aggravated type of the bilious remittent, and in such belief positively continued, as stated to me by Dr. King,\* until after the arrival of a communication to him from the British Superintendent of Quarantine.

Finally, Dr. King has come to the following conclusions (page 7 of report). "It becomes therefore a duty to express my opinion decidedly that there is no satisfactory proof of the disease having been propagated by contagion or from a specific poison, which is said to emanate from the bodies of the sick, 'the dying, or the dead'" (page 13). "And I must now declare my belief firmly, but conscientiously, that the Yellow Fever is not a contagious disease."

#### LETTER (S.)

In consequence of petitions from many citizens of New York, the House of Assembly appointed a Select Committee in relation to the Quarantine Laws, to assemble at Albany in 1845. The members of the Committee were D. E. Wheeler, C. Comstock, and R. H. Hine. Their Report is dated January 22, 1846.

At page 40 of the volume in which their proceedings are published we find the following observation:—

"The voice of mankind has called for relief, for in many instances quarantine regulations have been not only oppressive to commerce, but pestilential sources of disease and death."

Yellow Fever seems to have occupied the special attention of the Committee, and several respectable physicians of experience were called upon to furnish testimony as to the contagious or non-contagious property of that disease.

At page 171 of the proceedings, Dr. Hort, of New Orleans, states:—

"The facts, as presented by Dr. Beck, of New Orleans, in relation to the fever of 1822 in your city (New York), are of themselves sufficient to settle the question of contagion for ever; for the Yellow Fever cannot be contagious at one time and non-contagious at another; in short, the evidence that Yellow Fever is not a contagious disease, and therefore cannot be communicated by personal contact, is overwhelming. . . . I do not know a single physician in

\* From the same authority it appears that Dr. Almeida was a general merchant, who sold in his store a few simple drugs, and gave his advice gratis. It may hence be inferred that his opinions, even had they been fixed, could have been of no great value.

this city (New Orleans) who believes it to be contagious; and the public, with very few exceptions, are of the same opinion; and it would be difficult to find a physician of a different opinion at Havana, Vera Cruz, in the West India Islands, or, in short, in any part of the world where Yellow Fever prevails. We know of no malignant fever peculiar to tropical climates that is contagious; they all result from impurity of the atmosphere, occasioned by exhalations from the surface of the earth."

At page 197 :—

" Since the first time I saw a case of Yellow Fever, in 1822, to the present time, I have not seen the least evidence to prove that Yellow Fever is contagious; but facts have been constantly accumulating to prove the reverse. In this city my experience has been chiefly among the Irish and German emigrants; I have generally found them very much crowded, and particularly the Irish. In several instances, where, perhaps, 20 or 30 persons occupy two or three small rooms, and all unacclimated, I have attended five or six with the Yellow Fever, while all the rest, although in constant contact with the sick, have escaped the disease. How could this happen under such circumstances so favourable to the propagation of the disease if the fever were contagious?"

" There may be two or three physicians in this city who believe that Yellow Fever is contagious; but in opposition to this opinion the faculty may be said to be almost unanimous. Sporadic cases of Yellow Fever are utterly repugnant to the doctrine of contagion. Until within three years past, during which time the fever has been rather sporadic than epidemic, the Yellow Fever prevailed with regularity as an epidemic every other year, but there were always sporadic cases."

In the course of the investigation many observations for and against contagion were brought forward, but the necessary limitation to my Report prohibits my giving any further extracts from the proceedings than the following evidence of Dr. Reese, of New York :—

" The Yellow Fever never was, nor ever will be, imported into this port, by sea or land, in the persons of the sick, for the reason that it is not a contagious fever, as small-pox is—that it is not communicable from the bodies of the sick to the healthy. It can only be imported in the holds of vessels, nor is there any danger of cargoes or persons.

" The Yellow Fever is not communicated by personal contact, but by an infected atmosphere.

" No number of the sick can so infect a healthy atmosphere as to produce Yellow Fever, though other diseases may be produced by foul air; but if the air of a neighbourhood be infected by the cause of Yellow Fever, which is a specific poison, all persons breathing it any length of time are liable to be attacked, even those who only visit the infected district for a few hours. But in all cases the line may be readily drawn, circumscribing the infected district, and beyond which perfect impunity may be enjoyed, even though in contact with the sick and dying, who have been removed from the epidemic atmosphere. . . . And yet I have at another time lived and slept in the midst of the sick and dying of the Yellow Fever, who had been removed into a healthy situation, without witnessing a single instance of infection among physicians or nurses; and but for the antiquated superstition of the times, there would be as much reason for prohibiting a passenger whose leg was broken from being brought into the city as one sick with Yellow Fever. Indeed, there is quite as much danger of catching a broken leg in the one case as the Yellow Fever in the other."

## LETTER (T.)

*A List of 69 Men of the 43rd Regiment who were employed (in parties of from 3 to 6 daily) from the 2nd to 23rd October, 1828, in attendance on the sick, by night and day, at the Naval Hospital (Plan Letter D), for the period specified opposite their respective names.*

NAMES.	Date of Employment at Hospital.	No. of Hours Employed.	Posts, other Duties, Employments, &c., taken after their Hospital duty.	Date and Places of those subsequently attacked.	No. of Days which occurred between the date of the Employment of some of these Men and their being attacked with Fever.	Date of Decease of some of those attacked.
David Fenning . . .	2 Oct.	24	2 posts in town . . .	. . .	. . .	. . .
James Brown . . .	3 "	24	3 posts in camp . . .	. . .	. . .	. . .
William Bosworth . . .	3 "	18	" . . .	. . .	. . .	. . .
Arch. McHewey . . .	4 "	24	1 post in camp and town . . .	. . .	. . .	. . .
Thomas Cross . . .	4 "	24	Had resided for several days after in an infected locality.	Camp guard, 7 Nov. . . . .	33	. . .
Francis Clamo . . .	4 "	24	2 posts in town . . .	Camp, 22 Oct. . . . .	17	27 Oct.
George Cox . . .	5 "	24	1 post in camp and town . . .	. . .	. . .	. . .
Edward Dea . . .	5 "	24	2 posts in camp and town . . .	. . .	24	. . .
Jon. Cooper . . .	5 "	24	3 posts in town . . .	1 hour after coming off the town guard, 30 Oct.	. . .	. . .
John Mott . . .	6 "	24	2 posts in town . . .	. . .	. . .	. . .
William Taylor . . .	6 "	24	2 posts in camp . . .	In camp, 5 Nov., 3 days after last guard	29	. . .
Samuel Johnston . . .	7 "	24	" . . .	. . .	. . .	. . .
James Sample . . .	7 "	24	" . . .	. . .	. . .	. . .
John Ryan . . .	7 "	24	" . . .	. . .	. . .	. . .
David Officer . . .	8 "	24	" . . .	. . .	. . .	. . .
George Gardner . . .	8 "	24	3 posts in camp . . .	. . .	. . .	. . .
Thomas Walker . . .	9 "	24	3 posts in town . . .	4 hours after coming off last guard, 24 Oct.	. . .	. . .
William Warram . . .	9 "	24	3 posts in camp . . .	. . .	14	. . .
John Ried . . .	9 "	24	2 posts in camp and 1 in town . . .	On the town guard, 15 Oct. . . .	5	22 Oct.
George Loftus . . .	9 "	24	1 post in camp and 1 in town . . .	. . .	. . .	. . .
John Campbell . . .	10 "	24	Did not recollect . . .	. . .	. . .	. . .
Elias Balder . . .	10 "	24	2 guards in camp . . .	Day after last guard, 6 Nov. . . .	26	. . .

A List of 69 Men of the 43rd Regiment who were employed (in parties of from 3 to 6 daily) from the 2nd to 23rd October, &c.—continued.

NAMES.	Date of Employment at Hospital.	No. of Hours of Employment.	Posts, other Duties, Employments, &c., taken after their Hospital duty.	Date and Places of those subsequently attacked.	No. of Days which occurred between the date of the Employment of some of these Men and their being attacked with Fever.	Date of Decease of some of those attacked.
Jon. Cooper . . .	10 Oct.	24	3 guards in town . . .	“ “ “ “	“	“
William Gaining . . .	11 “	24	Did not recollect . . .	“ “ “ “	“	“
George Paul . . .	11 “	24	All guards in camp . . .	“ “ “ “	“	“
William Harbert . . .	11 “	24	2 guards in town . . .	“ “ “ “	“	“
William Haylock . . .	12 “	24	2 guards in camp and 1 in town . . .	“ “ “ “	“	“
Charles Mandefield . . .	12 “	24	“ “ “ “	On town guard, 21 Oct. . . .	8	8 Nov.
William Mills . . .	12 “	24	Colonel's orderly in camp . . .	In camp, 29 Oct. . . .	16	“
Thomas Sunott . . .	12 “	24	3 guards in camp and 1 in town . . .	2 days after town guard, eve 1 Nov. . .	19	“
John Williams . . .	13 “	24	3 guards in camp . . .	“ “ “ “	“	“
William Carroll . . .	13 “	24	“ “ “ “	“ “ “ “	“	“
James McMahon . . .	13 “	24	“ “ “ “	“ “ “ “	“	“
Patrick Ryan . . .	13 “	24	2 guards in camp . . .	“ “ “ “	“	“
John Billett . . .	14 “	24	2 guards in town and 1 in camp . . .	“ “ “ “	“	“
Michael Byrne . . .	14 “	24	2 guards in camp . . .	“ “ “ “	“	“
William Taylor . . .	14 “	24	“ “ “ “	“ “ “ “	“	“
John Barber . . .	14 “	24	“ “ “ “	“ “ “ “	“	“
John Bride . . .	14 “	24	1 guard in town . . .	In camp, 26 Oct. . . .	11	29 Oct.
James Cole . . .	14 “	24	“ “ “ “	In camp, 22 Oct. . . .	7	28 Oct.
William Demmick . . .	15 “	24	3 guards in camp . . .	“ “ “ “	“	“
John Goodwin . . .	15 “	24	“ “ “ “	“ “ “ “	“	“
Samuel Hillsden . . .	15 “	24	2 guards in camp and 1 in town . . .	“ “ “ “	“	“
John Felgate . . .	15 “	24	Orderly in town . . .	In town, 9 Nov. . . .	24	12 Nov.
John Fell . . .	15 “	24	2 guards in camp and 1 in town . . .	In camp 2 days after town guard, 28 Nov. . . .	43	2 Dec.
William Brown . . .	15 “	24	2 guards in camp . . .	“ “ “ “	“	“
Richard Claney . . .	15 “	24	1 guard in camp and 1 in town . . .	On town guard, 7 Dec. . . .	52	“
William McGowan . . .	16 “	24	“ “ “ “	“ “ “ “	“	“
James McCullum . . .	16 “	24	“ “ “ “	“ “ “ “	“	“

*A List of 69 Men of the 43rd Regiment who were employed (in parties of from 3 to 6 daily) from the 2nd to 23rd October, &c.—continued.*

NAMES.	Date of Employment at Hospital.	No. of Hours Employed.	Posts, other Duties, Employments, &c. taken after their Hospital duty.	Date and Places of those subsequently attacked.	No. of Days which occurred between the date of the Employment of some of these Men and their being attacked with Fever.	Date of Decease of some of those attacked.
Alex. McLaughlin . . .	16 Oct.	24	1 guard in camp . . . . .	. . . . .	. . . . .	. . . . .
Michael Kelly . . .	16 "	24	2 guards in camp and 1 in town . . .	. . . . .	. . . . .	. . . . .
Thomas Shaw . . .	16 "	24	2 guards in camp . . . . .	. . . . .	. . . . .	. . . . .
Edward Sapstead . . .	17 "	23	On fatigue in camp . . . . .	. . . . .	11	. . . . .
George Sloane . . .	17 "	24	2 guards in camp . . . . .	. . . . .	. . . . .	. . . . .
Thomas White . . .	17 "	24	1 guard in camp and 1 in town . . .	. . . . .	. . . . .	. . . . .
Bethel Knight . . .	17 "	24	2 guards in town and 1 in camp . . .	. . . . .	. . . . .	. . . . .
Thomas Rhoades . . .	17 "	24	3 guards in camp . . . . .	. . . . .	54	. . . . .
John O'Brien . . .	19 "	24	1 guard in town . . . . .	Civil camp guard, Dec. 11 . . . . .	3	. . . . .
William Horton . . .	19 "	24	1 guard in camp and 1 in town . . .	At Naval Hospital, when an officer's servant, 4 days, 21 Oct. . . . .	. . . . .	. . . . .
Maurice Fitzgerald . . .	19 "	24	2 guards in camp . . . . .	On town guard, 23 Oct. . . . .	3	. . . . .
William Fox . . .	19 "	24	. . . . .	3 days after town guard, 9 Nov. . . .	20	. . . . .
Thomas Beale . . .	21 & 22	48	1 guard in camp and 2 in town . . .	. . . . .	. . . . .	. . . . .
Joseph Venables . . .	21 "	24	1 guard in camp and 1 in town . . .	At camp, 9 Dec. . . . .	50	. . . . .
John Tale . . .	21 "	24	2 guards in camp . . . . .	. . . . .	. . . . .	. . . . .
William Beers . . .	22 "	24	. . . . .	2 days after town guard, 27 Oct. . . .	5	. . . . .
Michael Brady . . .	22 "	24	1 guard in camp and 1 in town . . .	. . . . .	. . . . .	. . . . .
Thomas Newman . . .	22 "	24	2 guards in camp . . . . .	. . . . .	. . . . .	. . . . .
Edward Green . . .	23 "	24	1 guard in camp and 1 in town . . .	. . . . .	. . . . .	. . . . .
Thomas McCasey . . .	23 "	24	" "	. . . . .	. . . . .	. . . . .

(Copy.)

I certify that the above-named men were employed as Orderlies in the Hospital of the 43rd Regiment Light Infantry for the periods specified opposite their names.

(Signed)

JONAH HARRIS, Lieutenant and Adjutant,  
43rd Regiment Light Infantry.

*List of Women who Washed the Hospital Dresses, Linen, &c., during the Gibraltar Epidemic of 1828, also for Officers who suffered from the Disease.*

Regiment.	Names.	Age.	When began to Wash.	For whom Washed.	OBSERVATIONS.
Royal Artillery.	Elizabeth Wilson . .	36	14 Oct. .	An officer . . . . .	Not attacked.
	Jane M'Culloch . .	40	Cannot be exactly ascertained.	" . . . . .	"
	Elizabeth Love . .	34	30 Oct. .	" . . . . .	"
	Servant, name unknown.	..		A lady who died . . . .	"
12th Regiment.	Eliza Slater . . . .	28	8 Nov. .	An officer . . . . .	"
	Bridget Foster . .	24	14 Oct. .	" . . . . .	"
23rd Regiment.	Ann Pager* . . . .	26	22 Oct. .	4 officers . . . . .	"
	Margaret Roberts* .	29	1 Sept..	2 officers and regimental hospital.	"
	Judith Bates* . . .	30	10 Oct. .	An officer . . . . .	Not attacked. The husband of this woman died of the disease.
42nd Regiment.	Ann M'Kenzie . . .	29	7 Sept..	Regimental hospital . .	Attacked 5th Oct., 28 days after commencing to wash. The subjects who suffered, living in the hospital district, were generally seized about the same time.
	Mary Connell . . .	28	24 Oct. .	Regimental hospital and 2 officers.	Not attacked.
	Jane Patterson . .	29	7 Sept..	12th and 42nd hospitals.	"
	Isabella Johnston . .	32	3 Oct. .	2 officers . . . . .	Not attacked; her husband died of the disease.
	Mary Deans . . . .	34	31 Oct. .	An officer . . . . .	Not attacked.
	Honorah M'Intosh . .	25	9 Oct. .	" . . . . .	Attacked 4th Nov., 26 days† after commenced washing.

\* A report existed that the two first of these women had attacks, but, on investigation, this proved unfounded; they never ceased from their usual work in consequence of sickness.

† Viz., a month after the disease appeared in the same locality among those who had not washed for the sick; her husband was attacked previously.

List of Women who Washed the Hospital Dresses, Linen, &amp;c.—continued.

Regiment.	Names.	Age.	When began to Wash.	For whom Washed.	OBSERVATIONS.
43rd Regiment.	Mary Finch . . .	30	12 Sept..	Regimental hospital . .	Attacked 2nd Oct., 20 days after commenced washing, and not till others in the neighbourhood were attacked.
	Ann Neesom . . .	26	7 Oct. .	2 regimental hospitals and an officer.	Not attacked.
	Nancy O'Niel . . .	43	12 Sept..	2 officers . . . . .	„
	Cecilia Pardoe . . .	27	„	„ . . . . .	„
	Ann Brown . . .	27	„	„ . . . . .	„
	Catherine Pegler . . .	24	5 Oct..	An officer . . . . .	„
73rd Regiment.	Nancy Peters . . .	26	1 Nov..	Regimental hospital . .	„
	Sarah Kelly . . .	36	14 Sept..	2 officers of the 94th Regiment who died.	Not attacked. The husband of this woman was attacked, all communication between them having been cut off at the time.
	Mrs. Coine . . .	..	Unascertained.	A lady . . . . .	Attacked 8th Oct., 24 days after commencing, at the same time with others in her neighbourhood.
	Mrs. Harris . . .	..	16 Nov..	An officer who died . .	Not attacked.
	Mrs. Ried . . .	..	6 Oct. .	2 officers and 73rd Artillery.	„
	Mrs. Watson . . .	..	18 Oct. .	An officer . . . . .	„
94th Regiment.	Mrs. Jones . . .	..	„	„ . . . . .	These women had slight illness in three or four weeks after beginning to wash, at a time when few of their class and neighbourhood escaped.
	Mrs. Woods . . .	..	Unascertained.	„ . . . . .	Not attacked.
Civilians.	Elizabeth M'Bride . .	..	18 Sept. .	4 officers . . . . .	„
	Nancy Skelly . . .	50	1 Sept..	73rd and 94th regimental hospitals.	„
	Maria Bernado . . .	..	20 Sept..	Civil hospital . . . . .	{ Slight illness 24 and 26 days after commencing, and when few in their district escaped.
	Estrudis Pasheca . .	..	15 Oct. .	„ . . . . .	}

(Signed)

J. GILLKREST, M.D.

## APPENDIX No. II.

## REPLY OF T. JONES HOWELL, Esq.,

*Judge Advocate, and Judge of the Vice Admiralty Court of Gibraltar, and one of the Members of the Board of Inquiry, instituted for the purpose of ascertaining the origin of the Yellow Fever epidemic of 1829 in that Garrison.*

ADDRESSED TO

SIR GEORGE MURRAY,

Then Secretary of State for the Colonies; who had requested (17 Sept. 1829) Mr. Howell to state his reasons for arriving at the opinion forwarded by him at the conclusion of the proceedings on that occasion, which was as follows:—

“UPON A CAREFUL REVIEW OF ALL THE PROCEEDINGS BEFORE THIS BOARD, I AM OF OPINION THAT THE EVIDENCE BROUGHT FORWARD HAS TOTALLY FAILED TO PROVE THAT THE LATE EPIDEMIC DISEASE WAS INTRODUCED FROM ANY FOREIGN SOURCE, EITHER BY THE SWEDISH SHIP ‘DYGDEN,’ OR BY ANY OTHER MEANS, AND I AM FURTHER OF OPINION THAT THE LATE EPIDEMIC HAD ITS ORIGIN IN GIBRALTAR.

*Gibraltar, 30th April 1829.*

T. JONES HOWELL.”

THE above is the conclusion at which I have arrived, after weighing the credit of the different witnesses, and the credibility of their evidence. To the expressions of the particular witnesses, and to the manner in which their testimony was delivered, I paid minute attention, and I took careful notes of their *vivâ voce* statements, at the very moment these were uttered. Having thus closely scrutinized the credit of the witnesses, their demeanour in giving evidence, and the character of the testimony itself, I am compelled to declare that I entirely disbelieve much of that testimony.

Two causes concurred to operate injuriously upon the proceedings of the Board: *First*, the conviction universally prevalent among the *civil* population of Gibraltar, that the prosperity of that community would be undermined if it should be proved that the epidemic had been generated on the spot, because of the prohibitions and restrictions which it was anticipated would in that case be inflicted upon its commercial intercourse with other places. Hence the notion that not only the last epidemic, but that all its predecessors had been imported from some foreign country was not only anxiously supported by the unanimous voice of the civil community, but it was with equal unanimity believed that a different doctrine would be fatal to the commercial prosperity of the place. From this feeling of self-interest it is to be admitted that

the *military* were exempt, a distinction between the two classes which ought to be taken into account in estimating the value of the evidence taken by the Board, and more especially the evidence of the medical practitioners.

The *second* cause operating injuriously upon this inquiry, was the publication, in the official Government newspaper (into which nothing is admitted except by official authority), on January 12, 1829, of an article authoritatively announcing that the late epidemic had been imported into Gibraltar, and denouncing as void of common sense any person who should hold a different opinion. This official notification of the feelings of the local Government (preceding as it did by only 12 days the appointment of the Board of Inquiry) could hardly fail to encourage evidence on one side, and discourage evidence on the other.

Bearing these circumstances in mind, I proceed to consider the evidence which was adduced to prove that the first case of the epidemic had its origin in some source foreign to Gibraltar.

That which was represented to the Board as having been the first case of the epidemic, came so represented by a low Spanish practitioner, Cortes by name, but better known by the class of people among whom he exercised his calling by the appellation of "Johnny the Bleeder."

The report of Cortes is in these words:—

"Return of the first case of fever under my treatment:—  
 "On the 9th day of September, 1828, Rosario Cortes, my  
 "young daughter, College Lane, No. 10,—Epidemic Fever.  
 "N.B. Some days before Dr. Braulio Lopez got sick, I was  
 "informed by him that the two sons of Felix Fenic, named  
 "Salvador Fenic and *Catalina* Fenic, had died of the black  
 "vomit, in district No. 24. The former died on the 17th of  
 "August, and the latter on the 20th of the same month.

"JOHN CORTES, Surgeon."

I am aware that by some who deny that the disease was imported, it has been asserted that these cases of Fenic's children were *not* the earliest cases of the epidemic; but no earlier case having been instanced to the Board, I shall assume that Salvador Fenic was the first patient afflicted with the epidemic, this being the position taken by those who assert that the epidemic was imported.

In examining the evidence which was given for the purpose of tracing the illness of Salvador Fenic to a foreign origin, it is necessary to be particularly observant of dates.

By the return of deaths in the Catholic persuasion, signed by the Catholic vicar, it appears that "Salvador Fenic, aged "13, died August 17th" (which will be found in that year to have been *Sunday*); "Catherine Fenic, aged 10, died August

"20" (Wednesday); and "Felix Fenic," the father, "aged 50," died not until "September 7."

Evidence was first adduced to the Board touching the history of this family on the 8th of April 1829, when Margarita Villalunga stated that she "lived in the yard of Fenic's house: "the boy took ill on Monday, and died the following Sunday." As we have seen that the day of the boy's death was Sunday, August 17th; this witness fixes the day of his attack to have been Monday, August 11th. "The girl took ill the day the "boy died," viz., Sunday, August 17th, "and died the Wednesday following," viz., August 20th. "I heard the boy say "that he, his sister, and his father," here is no mention, be it observed, of any other person, "had been on board a ship in "the bay on the Sunday, the day before the boy took ill," viz., Sunday August 10th; "the boy told me they had been on "board to eat, drink, and make merry on the Sunday: the "following day the boy was taken ill. I never heard what "nation that ship belonged to. I was *told by the boy* that his "father had *sold some tobacco* on board that ship, about an "arroba\* of cigars as a sample, and if approved, having about "30 quintals† of cigars, he had hopes of selling the whole to the "same ship."

This expression leads to the presumption that the ship, so alleged to have been visited, could not have been laden wholly, or in part, with tobacco. We must therefore attribute to some ship not so laden, but coming from within the prohibited degrees of latitude, the origin of the epidemic, if we believe it to have been imported by this family.

The witness added, "the father Felix Fenic, formerly had "been captain of a ship, and was latterly a manufacturer of "cigars." She was then examined as follows:—

"Do you know if he was owner of a boat in August last?— "His eldest son Joseph, about 22 years of age, was owner of a "boat at the time the other children were taken ill and died.

"Do you know whether the children that died went on "board in their brother's boat?—I do not know.

"*Did any body else go on board with them?—I do not know!*"

This last question and answer I consider to be material with reference to the subsequent evidence of the boy Caffiero. This question and answer not being set down in the minutes, I have transcribed them from my own notes.

The witness then described herself to have been employed by Fenic, as a sort of journeywoman, in the making of cigars, and the remainder of her evidence as delivered *at this time* (April 8th) in no way relates to the subject under consideration, viz., the introduction of the epidemic into Fenic's family.

\* An arroba is equal to 26 lbs. *Gib. price current.*

† A quintal at Gibraltar is equivalent to a cwt. *Ibid.*

The next witness examined upon this point was Cortes, the Spaniard already noticed. He stated that "Salvador Fenie, the son of Felix Fenie, about 12 or 14 years of age, was taken ill on the 12th of August, and died on the 17th. His sister Catalina Fenie, took ill on the day that the boy died. She died on the 20th August. Dr. Braulio Lopez *who is dead*," (these three words, which are omitted in the minutes, I have supplied from my own notes,) "informed me some days before he fell ill himself, that he attended these children, and that the disease of which they died was the reigning fever." But the witness Villalunga had previously stated that she was present when Dr. Lopez had pressed the girl's stomach and belly, and caused her to vomit black matter, and the doctor said *it was owing to her having eaten figs*.

I observe that the only important facts narrated by these two witnesses, they allege to have heard from two other persons, both of whom were dead at the time this evidence was given, and upon one important particular they are directly at variance.

The next witness upon this subject was the child Caffiero, whose evidence was given on the 10th of April. This boy was the first patient entered in the list of epidemic cases treated in the Civil Hospital. He is there stated to be 11 years old, and to have been admitted August 21 (Thursday). The story now told by this boy, is that he was in the habit of playing with Salvador and Catalina Fenie *every day before their death*; that he lived very near to them; that he was taken ill *two days before* he went to the Civil Hospital; that he saw Salvador and Catalina Fenie *every day* when they were sick in bed, the boy first, and the girl afterwards; that he saw the girl for the last time *on a Saturday*, "and the next day, Sunday, I fell ill myself; she was sick and in bed; the boy was then dead." These dates disagree with the journal given by Villalunga, of the events in Fenie's family; according to her, the girl was not ill on the Saturday, and the boy was alive on that day.

The only evidence which up to this period (April 10th) had been given to connect the illness in Fenie's family with a visit on shipboard, is the hearsay tale told by Villalunga, nor did she give to Fenie and his two children any companion in their alleged Sunday excursion.

Here this branch of the inquiry at that time terminated, and other matters were taken up.

Eight days after his examination above mentioned, the boy Caffiero re-appears as a witness (viz., April 18th) with a story entirely new, and which, if credible, would be extremely material; because he affects to speak of facts which had before rested on the hearsay evidence of Villalunga, but of which facts Caffiero now, after a lapse of eight days, represents him-

self to have been an eye-witness. On this his re-appearance, however, he carefully abstains from giving any date, either day of the week or month, or even season of the year. This cautious avoiding of dates may not unfairly be attributed to the variances between himself and Villalunga, in their respective journals of the illness of Fenie's children.

Caffiero now says, "I knew Salvo and Catalina Fenie, and "went on board ship with them; *I do not recollect* the day. "We went on board a three-masted ship. *I do not recollect* to "what nation it belonged. We remained on deck, and did not "go below. We remained on board about one hour. Fenie, the "father, took us on board; he rowed the boat himself; he ate "and drank on board, and then brought *a bundle of clothes on "shore.*" I have already shown that Villalunga ascertains the day of the visit of Fenie and his children on shipboard to have been Sunday August 10th. Caffiero now fixes the class of ship to have been one with three masts. On reference to the official return, I find that the only ship with three masts, not laden wholly or in part with tobacco, from any Yellow Fever latitude, on this 10th of August, was the Swedish ship "Dygden."

The boy, however, says nothing about selling cigars to the ship, nor had either he or Villalunga until this time said anything about a bundle of clothes.

This boy's second evidence thus proceeds:—"I did not understand the language of the people on board that ship; they "appeared to speak like Jews or Moors. I did not go on "board more than once. When we landed on the wharf, the "Maltese," *i. e.* Fenie, "*gave me some money, a pistoreen,\* and "told me not to say anything to anybody about our having been on "board.*"

"The effect which this was designed to produce is obvious, viz., that the ship visited was in quarantine, and Fenie, the Maltese, was conscious that he had committed an offence against the quarantine laws which rendered it necessary for his own safety that he should bribe this boy to secrecy. This story is full of incongruities; it is not probable that a man should select, for his Sunday excursion, to eat, drink, and make merry, a ship in quarantine; it is more improbable still that Fenie should gratuitously place himself in extreme peril, by taking with him (to be witnesses of his offence) children of the artless ages of 10, 11, and 13, on an expedition which in his own judgment, as demonstrated by his own act, he is convinced exposes him to severe punishment.

But with regard to the ship "Dygden," I find that she had already received pratique, and had been admitted to free intercourse with the shore, on the 6th of August, *four days previously*

\* The fifth part of a Spanish hard dollar.

to the alleged visit of *Fenie*, the date of which, notwithstanding Caffiero's loss of memory on his second examination, had already been ascertained by Villalunga to have been Sunday, August 10th, on which day *Fenie* therefore could commit no crime by going on board; and the story of the bribe and injunction to secrecy resolves itself into a clumsy and ill-disguised attempt at giving a colour of guilt to a fabulous occurrence which, even if it had been real, would have been guiltless.

Caffiero thus proceeds: "*I do not recollect* how long it was "after I came on shore from the ship that Salvador *Fenie* fell "ill;" and this he does not recollect, notwithstanding that,—if the *vivâ voce* diary given by Villalunga, of the proceedings in *Fenie's* family, tracing with mathematical precision effects to their causes, be true,—it must have been impossible for any one of the Sunday party of pleasure to disconnect the visit in the bay, and the consequent illness of *Fenie's* son on the following day; and more especially for one who, like Caffiero, had (as he said on his first examination) been in the habit of playing with Salvador and Catalina every day before their death, and who saw them every day when they were sick in bed, Salvador first, and Catalina afterwards, and who said he saw Catalina for the last time on a Saturday, "and the next day, Sunday, I fell ill "myself."

It is impossible not to mark the contrast between this boy's evidence on the 10th and that given by him on the 18th April; his discrepancies with Villalunga as to dates, when he did give them on his first examination, showed the inexpediency of committing himself to dates on his second.

His second evidence concludes thus: "My mother was a washerwoman, and washed for a black woman who lived next her. *Fenie's* wife refused to wash the bundle of clothes that he brought ashore; he offered them to my mother, who also refused them; he then gave them to an Englishwoman: I knew her: *she is dead: I do not know her name, nor where she lived.*" I find by my notes that he added, "This occurred during last winter," although the words are not entered upon the minutes. He was then asked, "What season of the year was it that you were on board of ship?" To which he cautiously replied, "It was either summer or winter, I believe."

Evidence such as this, and given as I saw it given, bears on its face every character of falsehood; and disbelieving as I do this boy's whole story, and at the same time considering his extreme youth, the testimony given by him has upon my mind the further operation of tainting with more than suspicion all the other evidence proceeding from the same class of witnesses, which consisted chiefly of hearsay in conversations with persons who had since died; because it would seem that this child must have been an instrument in the hands of some one of maturer age.

Further hearsay evidence—still hearsay from persons who are dead—came before the Board on April 24th. Josefa Salinas, who is described as the widow of a miner, then said, "I recollect that Felix Fenie's daughter, *who is dead*, came to my house *one day*, and *without my asking her*, told me that she had been on ship board with her father and brother. This was a little before the epidemic. The girl might be about 12 years old; *she gave me no particulars, as I asked her no questions*; she said she had been sea-sick. This she told me on a Monday," August 11, "saying that she had been on board the day before. She said that her father had been on board to sell cigars, but she did not say what ship." At the close of her evidence, this witness added, that she had had a conversation with Felix Fenie himself: "He told me he had been on board ship to sell cigars; that he had sold an arroba as a sample, and that he did not know whether he should sell any more. 'This is all he told me.'"

For the purpose of affording some collateral support to the second story told by Caffiero, the following evidence was given. On the 24th April Josefa Gonzales said, "I know Manuel Garcia, and knew the woman who lived with him; she was godmother to one of my children. I knew her mother, Catalina Barne, and her son, about five years old. *The two women and the child are dead*. They died during the epidemic; they lived in Chune Plamp's" (Fenie's) "yard, where the two children died, it was said, from eating green figs, about 15 days before the people were turned out from 24 District. Catalina Barne" (whom this witness had already stated to be dead), the mother of the godmother of my child, told me, before the people went out to the Neutral Ground, that the cause of the death of the two children had been the clothes brought from on board ship, and not the eating of green figs, as was reported; she did not tell me that these clothes were washed in Fenie's house. She only told me what I have already stated; and I accordingly requested Catalina Barne not to come to my house any more, *as I suspected there was fever in the garrison*." After giving a sketch of her acquaintance with Gibraltar epidemics, this witness added, "*She did not tell me that she had seen the clothes*; but she told me that *she believed* these clothes were the cause of the death of the children. I was told that Catalina Barne, her daughter, and her grandson died in A. M. Danino's-buildings. She merely told me that the clothes came from on board ship, but did not say from what ship." She was then questioned as follows: "Can you point out any person who can tell us where the clothes came from?"—"No; as I went out to camp, I cannot; *I wish I could*." This last expression denotes the animus with which this witness gives her evidence—if indeed such idle gossip, to use the

mildest term, can be treated as evidence at all, and if the retailing a rumour is to be considered as proof that it is true.

Josefa Salinas was asked, on the same day, "Do you know anything about clothes having come from on board ship to the house of Fenic to be washed?"—"I do recollect having been sent for by the mother of the Fenic children to wash some clothes, as I am a washerwoman; I did not go, as I was then washing in the house of Mrs. Magdalena Roca. I received the message the Wednesday," August 13, "next after the Monday that I had the conversation with the Fenic girl."

"Who brought this message to you? I received two messages; the first brought by Serafina, a girl then living in Fenic's house; the second message, *the same evening*, by Antonio Jonè, a Mahonese; both these individuals are now alive; both messages were for me to go and wash some clothes; the second messenger said if you cannot come *to-day* come to-morrow." We have seen that these messages were delivered to the witness on the *Wednesday (August 13th)*; but unfortunately for her story, however circumstantial, the Swedish ship 'Dygden' had sailed to Cadiz on Tuesday (August 12th), one day before these pressing messages were sent to the witness.

On the same day (April 24th) Margarita Villalunga re-appears as a witness, after an absence of 16 days from the Board, during which interval she had ample time to revise her evidence and to follow out the tale broached by Caffiero on his *second* appearance. She now remembers that "On Sunday the children told me that they had been on board that day, and Monday the first of them fell ill. On Wednesday" [We have already seen that the "Dygden" had gone to Cadiz on the preceding day] "Mrs. Fenic asked me to wash some clothes, which I did not do, being indisposed myself. I know that Mr. Fenic asked another woman who washed for Mrs. Magdalena Roca, with whom I now live, to wash these clothes, which she could not wash, being engaged. I was told by Mrs. Fenic that she put these clothes out to be washed." I have observed that Caffiero, after eight days' absence from the Board, added to his original testimony so much as to give to it a new character altogether; I now observe, that six days after Caffiero's amended testimony, and sixteen days after her own original examination, the woman Villalunga comes back with a new story, of which singularly enough the principal point is made to coincide with the alterations and emendations in the evidence of Caffiero.

John Nichols, journeyman tailor, appeared on the same day (April 24th) as a witness, and gave to the Board a new reading of the story respecting the visit to the ship in quarantine. His version runs thus: "I knew the Fenic family. I was their

"neighbour for some time. The eldest son was a ferry-boat man. One day he came to me with a pair of pantaloons belonging to him to be repaired; he said *he had been nearly drowned*, his boat having been upset *coming from a vessel in the Quarantine-ground*: this was about *three weeks before the death of his brother and sister*. I saw the children carried out dead from the same house within three days of each other. I was told they had died of eating green figs; but having been here in the epidemics of 1813 and 1814, I suspected that they had died of the same kind of fever, and reported the circumstance to the late Serjeant King, who was Police-Serjeant of that district. I knew the father of the Fenic family; he was a cigar-maker, and had formerly kept a bum-boat."

On 25th April, Antonio Jonè, water-carrier, informed the Board that he was sent by Mrs. Fenic to fetch a washer-woman, Josefa Salinas, at the time Mrs. Fenic's children were ill. "I know nothing about the *clothes* that were to be washed, nor where they came from."

The only remaining evidence upon this point goes to negative the supposed visit of Fenic and the children on ship board, as well as the supposed connexion between that visit and the washing of clothes.

Mr. Leach, clerk in the house of Cosens and Co., stated that he went on board the ship "*Dygden*" the Sunday after her admission to pratique; now, as she received pratique on the 6th of August, Mr. Leach's visit must have been on Sunday, the 10th, the very day which has been assigned for the visits of the Fenics and Caffiero. Mr. Leach said he remained on board from 10 o'clock on that day till between 12 and 1, and that he saw no strangers on board, except his companion Mr. Travers. As the boy Caffiero had said that Fenic and the children *remained on deck* the whole time they were on board, it follows that if they had been on board at the time spoken to by Mr. Leach, he must have seen them. But as we have seen that Caffiero abstains from fixing any time at all, the evidence of Mr. Leach amounts to little.

Victoria Ferrari (who is called by preceding witnesses Serafina) states, that she lived close to Fenic's family before the epidemic; that she used to go there to wash Mrs. Fenic's clothes and her own at the same time, and occasionally to assist in the house during Mrs. Fenic's sickness. With reference to the evidence of Josefa Salinas, this witness adds that she was sent by Mrs. Fenic long—some months—before the epidemic, to ask Salinas to wash for her. "I was sent only once, and Salinas did not come on that occasion. *The things to be washed were clothes belonging to the house*; there were a good many things, as Mrs. Fenic had been ill for some days; when she got better, she and I washed these clothes—this was long

“before the children fell ill. Fenic’s eldest son kept a boat. Caffiero used to play sometimes with Fenic’s children, after their cigar-work was over. I have had no conversation with any of the Fenic family about my examination here to-day. I do not speak to the mother, and if I spoke to any of her sons, it was not about this.” She was then asked, “Are you about to be married to any of the sons of the late Felix Fenic?” and it is said that the witness seemed to intimate that she was. This question was evidently put to impeach the credit of the witness by imputing to her a motive for concealing Fenic’s alleged connexion with the importation of the epidemic, lest it might bring upon the surviving but innocent members of his family the execrations of the community of Gibraltar. I do not believe that any interested motive such as I have pointed at could exist; or that it would operate to the prejudice of the family of any man, that evidence should be given of his having introduced the last or any former epidemic. So far as I can judge from my own observation, no popular ill-will would be to be apprehended by any man who should furnish testimony of an epidemic having been imported by some member of his family. On the contrary, the pertinacious adherence of this woman and of the widow of Felix Fenic to their assertions, in opposition to the wishes of the public, would be more likely to expose them to popular indignation, than would be their acquiescence in any of the stories related by the other witnesses.

To show that practically there existed at Gibraltar no reluctance to meet the popular wish by avowing a share in the importation of an epidemic, some instances occurred before the Board. Thus Francisca Roca gives evidence to show that the last epidemic was introduced by a person whom she states to be a sailor; she does not indeed know where this sailor is to be found, nor in what month, nor in what ship he came to Gibraltar, but she “thinks he is at sea.” Her ignorance touching his movements is the more remarkable, because she represents him to be her own brother. Francisco Roca declared himself to be the father, and Diego Fabrica declared himself to be the uncle of this absent mariner; and all presented themselves as ready witnesses on behalf of their relative to establish his claim to be an original importer of the last epidemic. It was stated in support of the pretensions of this invisible claimant, that he had come to Gibraltar in the “Dygden,” and had been there discharged; but unfortunately the official return of sailors discharged from that vessel, furnished from the Swedish Consulate, included not his name. As I shall have occasion hereafter to expose further the absurdity of this claim, I quit the subject for the present.

Another instance is a man of the name of Santos, to whom

the introduction of the epidemic of 1804 is attributed, which Santos is still in Gibraltar, and is far from suffering in public estimation in consequence.

The only remaining witness concerning the introduction of the epidemic into Felix Fenie's family, is the widow of Fenie himself. She was examined on 25th April. She was, at my desire, particularly reminded that the duty which she owed to society required her to disclose everything that she knew, and from the ingenuous manner in which her evidence was given, I am led to believe that she spoke the truth.

She declared that she did not know the cause of her children's illness: "they were attended by Dr. Lopez, who is dead, and "who said they had a tabardillo and indigestion, *caused by "eating green figs.* He did not say what was the cause of the "tabardillo. My husband was a cigar-maker; but he did not "go on board ship either to buy tobacco or to sell cigars. "Neither my husband nor my children went into the bay at "any time during last summer or autumn. I know this, "because if they had gone, they would have told me, and they "did not tell me."\* Nor indeed is it to be supposed that the children would not have told their mother, and that the husband would not have told his wife, that which all of them are declared to have communicated so freely to other people.

Finally with respect to this family, it will be seen by a communication addressed by Dias, a Spanish doctor, on January 30th, 1829, to the Secretary of the Board, that Dr. Lopez, who attended Fenie's children, attributed their illness to eating green figs, and who, being asked whether, had they eaten green plumbs, the effect would have been the same, the Doctor drew a scientific distinction between the action of the two fruits. The whole letter of Dias is curious as illustrating the science of the Spanish practitioners at Gibraltar.

Having thus examined in detail the evidence adduced to connect the illness of Salvador Fenie (the alleged first case of the epidemic) with the "Dygden,"—and no other vessel has been pointed at,—I find not only that it completely fails to make out even a *primâ facie* case, but also, from the whole complexion of the evidence, I am convinced that the story of Fenie's visit to that vessel on the 10th of August is, from beginning to end, a fabrication.

In anticipation of a failure to connect the illness in Fenie's family with a foreign source, much testimony was given, through channels most impure, about instances in which foul clothes are supposed to have been brought ashore by sailors arriving from the Havannah, in the early part of the epidemic, and which foul clothes infected the washerwomen.

Mary Parody, a woman whose claim to veracity no one

\* See also Dr. Gillkrest's Report. Addenda H., p. 223.

attempts to uphold, declared that she "went to Mrs. Sileox's "to wash with her some clothes which were full of vomit;" during which washing she (Parody) fell ill. The next day she returned to Sileox's and found her ill in bed. Parody then delivered the clothes, which herself and Sileox had the day before washed, to two sailors, who, although requested to wait until the morrow, said *they could not wait, because they were going away to Cadiz*; and they accordingly took away the clothes, still wet from the tub, *on board ship*. Sileox told Parody that the clothes belonged to *the Swedish ship* which had come from the Havannah, and that on the passage nine men had been sick and three had died. Parody took to her bed the day she delivered the wet clothes to the sailors. *She had been six days in bed when Mr. Duguid's coachman was buried*. Five or six days during her illness she was seen by *the late Dr. Lopez*. When she was getting better she went to the Civil Hospital, but returned the same day.

Such is Mary Parody's story; but perverse unaccommodating dates destroy its effect. Mr. Duguid's coachman died September 1st, and supposing him to have been buried September 2nd, Mary Parody must have, *at the very earliest*, taken to her bed on August 26th, having delivered on that day the wet clothes to the sailors of the Swedish ship. The Swedish ship, however, had sailed from Gibraltar for Cadiz on the 12th of August, which circumstance presents an anachronism fatal to the narrative of the witness.

The steward of the Civil Hospital fixes the day on which Parody presented herself there to have been August 30th. He was present when she stated her complaint to the doctor. She complained of a disease of the womb, for which Mr. Wilson gave her a powder, *but did not admit her into hospital*.

Anthony Francis, a blacksmith, states that although Parody told Dr. Woods, a gentleman who appears to have seen her before she went to the hospital, that her complaint was a disease of the womb, she confidentially told Francis that if she acknowledged the nature of her illness to Dr. Woods, he would send her to the hospital, whither in fact it appears that she ultimately went, although they would not accept her as a fever patient.

In corroboration of Francis' evidence, Mary Galt, a person of the same class as Parody herself, appeared and gave her medical opinion that Parody's disease was the epidemic fever. Parody herself stated that she had been in Gibraltar in all the epidemics. In that of 1804 all the family, *except herself*, were ill; in 1810 two died in the house in which she lived, and the rest were immediately removed to camp. In the epidemics of 1813 and 1814 *no person* in the house in which she lived was ill.

Angela Bebraqua, Parody's mother, declared that Parody had not the epidemic in 1804, in 1810, in 1813, or in 1814.

Mr. Amiel, however, the surgeon of the 12th Regiment, gave the following evidence upon this subject:—"I know Mary Parody; she lived in 1810 close to Mr. Rey's family, in the South. Her mother paid great attention to the Rey family during their illness, and the Parody family were consequently removed with the Rey family and the other neighbours to the Neutral Ground, by order of Dr. Pym. These families returned to the same buildings after about 16 or 17 days; two people fell sick in those families after their return; and the Rey family were again sent out, but the Parody family was not sent out, as Mr. Ferrari, the owner of the house and inspector of the district assured me, and I assured Dr. Pym, that the Parody family had passed the fever in 1804." I have taken this evidence from my own notes, which are somewhat more full than the minutes.

I have gone into greater detail upon this evidence respecting Parody and Silcox than I should have thought necessary, because the cases were strongly relied upon at the Board, although the anachronism above pointed out at once overturns them; independently of which, we have the evidence that Parody's disease in 1828 was not the epidemic fever, and the presumptive evidence to show that Parody had the epidemic fever in 1804. With regard to Silcox, she was the woman whose extraordinary seclusion was so pointedly noticed by the late Dr. Hennen and by Mr. Woodward, in his *vivâ voce* evidence.

Another channel through which the epidemic was stated to have been imported from the Havannah is by means of the clothes of Teste, the health guard, which on his return from the "Dygden" were washed by his sisters, and, it is assumed, infected one of them with the Yellow Fever. This story was introduced by the Spanish practitioner Dias, who in a letter dated January 30th, addressed to the secretary, states that he has no doubt that Miss Teste's disease, for which he was called to attend her on the 24th August, and with which she had been attacked August 21st, was the Yellow Fever. Dias's letter upon this subject is worth perusal.

Teste, the health guard, states that on his return from the "Dygden" his clothes were washed by his three sisters on the 10th of August; his sister Mary was taken ill on the 21st: "She was not ill of the epidemic fever, because I heard the Spanish Dr. Dias, who attended her, tell the Spanish Commission from Algeçiras, that it was a common bilious remittent fever." Teste himself was ill of the epidemic from the 11th to the 26th of September, more than five weeks after he had quitted the "Dygden," and three weeks after the washing of the clothes which Teste had worn on board that vessel had, as it is said, infected his sister. Dias himself was

then called, and said he had set Miss Teste down as suspected of labouring under Yellow Fever, which suspicion afterwards amounted to a certainty. He denies that he made any observation as to the nature of Miss Teste's disease to the Algéiras Medical Commission, he "merely showed the patient, as they had eyes as well as I."

Teste being recalled, repeated his first statement, and Dias in like manner persisted in his denial. Dr. Massias, one of the Algéiras Commission, on a subsequent day, came over to Gibraltar, and declared that he saw Miss Teste with Dr. Dias in September, and that on that occasion "Dr. Dias told me that she had a simple affection of the stomach, for which he was then giving her a watery infusion of bark." Dr. Gillkrest, 43rd Light Infantry, added to this, that he was present when on September 4th all the civil medical practitioners were assembled in the Town Major's office, and were requested to give frankly, for the information of the Medical Commission from Algéiras, an account of the nature of the fevers they had had under their charge during the summer. "All declared that up to that period no fever had been treated by them, except what they called *estacionales*, meaning fevers from local causes, and at particular seasons of the year. I recollect perfectly well that Dr. Dias was present on the above occasion. I know that he in particular made a declaration to that effect; in fact, as he was the leading man, I took particular notice of him. He, as well as Dr. Mortera, stated the same thing to me afterwards." I cannot but observe that this is not the only instance in which the evidence of Dias is contradicted. Evidence was then given to support the testimony of Dias, by proving repeated unsolicited declarations by *this health guard* Teste, that he himself had brought the epidemic into Gibraltar from the "*Dygden*." It is only necessary to read this evidence, especially that of Picardo, to be convinced of its futility, bearing in mind the public office which Teste filled at the time.

I shall shortly notice the absurd detail of Mr. Beneditto Cohen, who is described in the Minutes as a "public writer." This writer pronounces that his daughter had the epidemic and caught it by watching and stirring some clothes which were being boiled; and he gives as a diagnostic of the disease that which appears to have escaped the professional men whose evidence was taken upon the medical topics that were inquired into. Having declared that his daughter so fell ill, and that he himself fell ill immediately after, in order to bring their disease under the proper class, he describes a very simple symptom by which to detect its species, and (what is more important) he points out, with the assistance of Cortes, an equally simple cure. After giving a long detail about a female, whom he describes

as "a favourite woman of Mr. Horsey's," whose name is unknown, but who is said to have the nickname of Moma, who was a washerwoman, and had asked this public writer's daughter to watch and stir some boiling clothes for her during her absence; immediately after which Moma fell sick, then Miss Cohen, and lastly Cohen himself. After informing us that the nameless Moma had told him that these clothes which herself had washed and his daughter had washed and stirred, had come from some ship from the Havannah. This witness proceeds thus:—

"Finding a *taste of mutton* in my mouth, and that my daughter had the *same taste of mutton* in her mouth, the second day after my daughter was taken ill, being the morning I was taken ill myself, I sent to Mr. Cortes's shop and bought two doses of salts, one for myself and one for my daughter, thinking that this was the most proper medicine. Finding that this medicine, one ounce and a half to myself and one ounce to my daughter, had no effect on either of us, both getting worse, *with a taste of mutton in our mouths*, the next morning I called again at the same apothecary's shop, scarcely able to walk, and with a burning fever. I found the doctor, and got a vomit of hippo for myself and another for my daughter. He advised me not to take a vomit, or I should die directly, but to take a clyster. I was convalescent on September 3rd, after I had been ill from the Saturday night till the Friday following." This man and his daughter were immediately afterwards sent with the other persons of his neighbourhood to the Neutral Ground. I have copied this part of his evidence from my own notes, because this public writer's grave reference to the *taste of mutton* has been omitted in the Minutes, and his whole relation upon this point, to be properly appreciated, should be taken together. And this man's evidence, with a declaration that the nameless Moma died during the epidemic, and was in the habit of washing ships' clothes,—which declaration is made by the same Mary Galt, whose profession need not be adverted to;—this is the evidence upon which I am seriously called upon to say that it is proved that such a person as the nameless Moma washed clothes from the Havannah,—that this washing infected her with Yellow Fever,—and that the watching and stirring the same clothes gave to Miss Cohen the Yellow Fever, and which disease she communicated to her father.

In failure of all these attempts to trace the epidemic to the Havannah, through the washerwomen Parody, Silcox, Teste, and the nameless Moma, one other effort to fix the fever upon the "*Dygden*" remains to be considered. This effort was made by Francisca Roca, who represents herself to have had a brother on board a ship from the Havannah, who had had the fever

there. He brought clothes ashore, which she threw out of the bag, *but did not wash*. Four or five days afterwards she replaced them in the bag and returned them to her brother, who took them away and embarked in another vessel. "Thirteen or fourteen days after he went away I fell ill; I never counted the days. I kept my bed four days." She does not know the month in which he arrived, nor the vessel he came in. She does not know where he now is, but thinks he is at sea. "I was ill once with the epidemie, but no more."

Mr. Charles Bellardo, the master of the last witness, tells nearly the same story, adding, that the ship in which Roca, the sailor, arrived was the "*Dygden*." The only difference between them is that, according to Mr. Bellardo, Franeisea fell ill on the 20th of August, two (instead of 13 or 14) days after her brother's departure, and had "one day's fever, and two days not well." His evidence then proceeds in these words: "When she took ill, at that time Dr. Mortera said it was a liver complaint; but some time after, *when it was spread about that the "Dygden" had imported the fever*, Mr. Mareos Superviele persuaded both Dr. Mortera and myself that he, Dr. Mortera, had cured Franeisea Roca of the Yellow Fever." Without meaning to insinuate aught against the persuasive powers of Mr. Mareos Superviele's eloquence, it must, at least, be admitted that, in the present instance, one-half of his audience was well disposed to be convinced by his arguments. Mr. Bellardo proceeds with admirable naïveté: "When she was sick again on the 28th of September, both Mr. Superviele and Mr. Mortera, and myself, were disappointed about her first sickness. Mr. Mortera attended her in both illnesses; this last time she was in bed six or seven days."

It would be an inexcusable waste of time to pursue this further, and follow out the desperate attempts to show that some three weeks after this sailor had quitted Gibraltar, and when the epidemic was raging through the town, some children, who then caught the epidemic, must have contracted it from this man, because, amongst other reasons, they had been in his company and played with some tools three weeks before, when he was sawing some planks.

This Roca is the man whose name, as I have already stated, does not appear in the official list, furnished by the Swedish Consul, of sailors left by the "*Dygden*" at Gibraltar.

Here I leave the journals of washerwomen, and the tattle of their gossips, remarking this fatal objection to each washing-tub anecdote, however circumstantial, that *not one of them goes back so far as to precede*, and therefore to account for, the alleged first case of the epidemic, namely, that of Salvador Fenie, who, as we are told, fell ill on the 11th of August, and upon whose single case, therefore, the proof of importation rests. And if

the attempt to connect the illness of Salvador Fenic with a foreign source be, as I hold it to be, a complete failure, how is the illness of the boy Caffiero to be accounted for? And to what is to be ascribed the illness of Mr. Martin's child on August 16th, a case quite as early as that of Caffiero, and which has not been attempted to be traced to importation? not one of the washing-tub cases being anterior either to that of Mr. Martin's child or to that of Caffiero, both of which are unquestioned cases of the epidemic.

I find nothing like satisfactory proof that a disease identical with the epidemic existed on board the "Dygden." On the contrary, while that vessel was lying in quarantine at Gibraltar rumours against her had been circulated, and, in consequence, an extraordinary quarantine had been imposed at Alicant upon vessels arriving from Gibraltar. This was known at Gibraltar at the end of July; was officially announced by the Lieut.-Governor to the late Dr. Hennen, who, in his letter of August 2nd, expressly states that he had *minutely inspected* the captain and crew of the "Dygden," whom he found *in perfect health*, adding, "I shall repeat my inspections before the expiration of their quarantine, the 6th of the present month." This report was written, as it strikes me, under circumstances which entitle it to much consideration. This ship had been officially pointed out to him (as the medical officer of quarantine) as being strongly suspected. The responsibility of his office was thus brought fully before his eyes, and he had *then* no motive for making a false report of his inspection of the "Dygden's" master and crew, because the epidemic had not at that period commenced. If he had observed any reasonable grounds for suspicion, he had only to fall in with the rumour and recommend that none of the persons or susceptible articles on board should be permitted to land. The conduct and declarations, therefore, of Dr. Hennen, as a responsible public officer, under such circumstances, when, if he erred at all, it would probably be on the side of *over caution*, I hold to be most material.

The inability to make out that the epidemic was imported, notwithstanding the exertions which were made, under such favouring circumstances, for that purpose, I take to furnish a presumption that the disease was indigenous. I am aware that it is thought that the circumstances of the introduction of a disease are difficult of proof. I do not agree in this. I think, on the contrary, that if the stories of Villalunga and Caffiero had been consistent with truth, in the absence of countervailing evidence, their testimony would have been sufficient to establish the position.

It has been, upon insufficient evidence, assumed as a fact, notwithstanding the "Dygden's" bill of health (a very

important document which, although presented to the Board and ordered to be attached to the proceedings, is not there to be found),\* that a disease identical with the epidemic existed at the Havannah when the "Dygden" sailed. It would, however, not be unreasonable to presume that at some period or other, between the years 1815 and 1827, both inclusive, the Yellow Fever existed in the latitudes appropriate to it. By a return laid before the Board, it appears that in those 13 years not fewer than 844 vessels arrived at Gibraltar from those latitudes, some having lost men, some having been sickly on the voyage without losing men, although none actually having patients on board labouring under Yellow Fever on their arrival at Gibraltar. It appears by the evidence of the Deputy Pratique Master that he does not recollect, during the 18 years he has held his office, one single instance of health guards being put on board, or of fumigation being applied to any ship from the West Indies before the "Dygden" (an admission which would seem to be perfectly disinterested on the part of the department to which this gentleman belongs), although he states that he thinks he has never seen the crews of vessels which have arrived from the West Indies in perfectly good health on arrival. In many cases all on board have borne a sickly appearance, while the precautionary measures adopted in those years were much less rigorous than those adopted in the case of the "Dygden," for that, subsequently to 1814, such vessels were allowed to bring cotton, which they now are not; and he only recollects one instance (which could not have occurred between 1815 and 1827) in which the bedding and clothing of persons on board ships in which sickness had prevailed, during their voyage from the West Indies, had been immersed in sea water. He attributes the extraordinary measures resorted to in the case of the "Dygden" to a knowledge of the rumours which had been spread on her arrival. Now, under the very liberal pratique regulations which obtained in the 13 years between 1814 and 1828, with regard to sickly vessels arriving from Yellow Fever countries, and considering the extensive intercourse between those countries and Gibraltar, in every season of each of those years, it strikes me as being impossible that a Yellow Fever epidemic should not have been imported during that long period, if it be importable.

The facts stated by Lieut.-Colonel Bayley, the commanding officer, and by Mr. Amiel, the surgeon of the 12th Infantry, are, in my opinion, conclusive. That regiment, in the epidemic of 1828, presented a singularly good opportunity for trying some important questions.

When the sickness had appeared in that corps the regiment

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\* See Dr. Gillkrest's Report, Addenda (F.), pp. 214-16.

was removed to the Neutral Ground from its quarters in the town, one range of which adjoined the district first infected. This removal took place on September 5th, at which time the regiment had sent five patients into the Regimental Hospital. This corps was exonerated from its share of military duty in the garrison, a part of which had been the furnishing guards in and about the infected district. No new case of fever occurred in the corps while it remained on the Neutral Ground, although several orderly men attached to the different departments in town, *who had not been removed* with their regiment to the Neutral Ground, caught the disease. Lieut.-Colonel Bayley says, "On the 23rd September we commenced taking the town duties again, and on the 25th men began to go rapidly into hospital, from which time this continued until the termination of the epidemic:" 282 women and children of that regiment, who continued on the Neutral Ground from the 5th September until the termination of the epidemic, wholly escaped the disease; one woman only, who having re-entered the town and remained in it a day or two, caught the fever, and she died. Several of these women slept on the Neutral Ground in the same beds with their husbands, while these last were labouring under the epidemic which they had contracted after the regiment had resumed its share of duty within the garrison, and with their children continued to use the same bedding after the husbands were removed to hospital; but in no instance was the disease contracted by the wife or child of the soldier, who had thus caught the fever during his temporary absence from his tent on the Neutral Ground, while mounting guard in the garrison.

Four orderly men, who attended several fever patients in the hospital sheds *on the glacis* in front of the northern extremity of the Rock, and above the Neutral Ground, escaped from any attack; but not one of the orderly men employed during October in the hospital *on the Rock* escaped, excepting one of the watermen.

The fever spread among the population in that part of the Rock, called "the South," two or three weeks later than in the town; from September 2nd to October 1st, several cases of the epidemic were admitted into the 12th Regimental Hospital at "the South," three of which died with the Black Vomit; but the disease never attacked any of the other patients, more than 20 in number, treated in that hospital at the time for other complaints, nor any of the orderly men who had incessant and unreserved intercourse with the fever patients, were frequently in contact with the dying, and slept in the same wards.

It was only on the 25th September when the disease had spread to "the South," and the epidemic influence had extended through the district where the hospital is situated, that the

cook, *who never had occasion to approach the sick*, contracted the disease; and in October, when the atmospheric causes had acquired more intensity, the hospital serjeant and 12 orderly men sent into the hospital at the South, from the camp on the Neutral Ground, were taken ill, but a few days after entering the precincts of the hospital, and several of them died.

These are facts which, proved, as they are, by the unimpeachable testimony of the lieutenant-colonel and surgeon of the 12th Regiment, in my opinion irresistibly force upon the mind the conclusion drawn by those gentlemen, that the origin and spread of the disease were local, being confined within the limits of the vitiated atmosphere of the place.

If anything could add to the force of such evidence it would be that which followed:

Upon the removal of the 12th from the Rock to the Neutral Ground, the duties theretofore allotted to it were taken by the 43rd Light Infantry; which corps, however, be it remarked, did not occupy the barracks which had been vacated by the 12th.

"The two *first* men of my regiment," says Lieut.-Colonel Haverfield, commanding 43rd Light Infantry, "were taken ill "on the 10th September, a third on the 11th; after which "there were admissions almost every day into hospital until "the epidemic ceased." Those guards from which the 12th had been relieved were furnished exclusively by the 43rd until September 17th, when this regiment also marched into camp on the Neutral Ground, and were succeeded in the whole of these duties by the 42nd Royal Highlanders, the first case in which regiment occurred on that day, as appears by the evidence of the commanding officer, Major Middleton; and when the 42nd Royal Highlanders marched out into camp on the 22nd September, 18 cases of fever had occurred in it. On the 23rd September the 12th Regiment resumed its share of the town duties; from that time these duties were taken by the three regiments in common, and we have already seen by the evidence of Lieut.-Colonel Bayley, that from that day the fever, which had ceased in the 12th for 18 days, re-appeared, and continued to rage till the termination of the epidemic.

It is impossible to imagine a chain of circumstantial evidence more perfect in all its parts than this, or facts established by witnesses more disinterested or of higher character.

Much conflicting evidence was given upon a question of fact, namely, the occurrence, in seasons when the epidemic did not prevail, of scattered cases of the same disease. This question of fact became practically a question of science, because the medical members of the Board disputed the identity of these occasional scattered (or, in medical phraseology, sporadic) cases with the epidemic disease.

It is an extremely delicate matter for a man, unskilled in the art of medicine, to determine between the conflicting opinions of medical men and to decide upon the facts adduced by them to support their theories. I approach this part of the subject, therefore, with much diffidence; and the more so, because a line of examination, which I felt myself unable to follow, was pursued in regard to some few of the medical witnesses, which was excellently calculated to try their science and skill, partaking, as it did, in a great degree of the character of a severe academical examination. This line of examination having been pursued in some instances, I regret that there was no medical member on the Commission who held the opinion that the epidemic was indigenous, so that the same mode of treatment might have been applied to witnesses *on both sides*, that all the facts adduced should have been subjected to the same anatomical dissection, and that the opinions of both parties should have undergone the same public medical scrutiny; that Doctors Ardevol, Dias, Zela, Cortes, Bobadilla, &c., should have had the same opportunity of vindicating *their* professional opinions, under a skilful medical cross-examination, which was afforded to Mr. Wilson, Mr. Fraser, Mr. Amiel, and Dr. Chervin.

Notwithstanding, however, that this course of professional examination was confined to one class of the medical witnesses only, so that on a point where science and skill form the only grounds of confidence, the science and skill of every professional witness were not subjected to the same severe test, and cannot, therefore, be equally appreciated, I must deal with the testimony as I find it, remembering that my judgment is to be guided by the weight of the evidence, and not by the number of the witnesses.

Several medical gentlemen depose to having seen cases identical with the epidemic disease of Gibraltar, in seasons not epidemic.

Mr. Amiel, surgeon of the 12th Regiment, has constantly resided in Gibraltar from the year 1810; and it appears that his experience of the Gibraltar epidemic is by no means inconsiderable, for in 1810 he treated about 12 patients; in the epidemic of 1813, about 200; in that of 1814, about 200; and in that of 1828, about 260. This witness was asked,—

“From the beginning of 1816 did you meet with any cases  
“of fever here which you consider to have been identical  
“with the late epidemic?”—“I have met with some cases of  
“fever here which I consider *perfectly* identical with the late  
“epidemic.”

“How many sporadic cases of this disease have you met  
“with? in what years? and in what months of the year during  
“the period alluded to?”—“In my opinion I have seen five  
“distinctly marked cases during that period; one in the month

“ of August, 1818; two in September, 1822; and two in November, 1825, *one of which is given in my report for that year.*”

“ Will you be good enough to describe the symptoms by which you consider that these cases were distinctly marked cases of a disease identical with the late epidemic?”—“ They were attended with very high fever, great irritability of the stomach, yellowness of the skin, black dejections, and in two of these cases the black vomit. The two with the black vomit died, and in the three who recovered there were passive hæmorrhages.”

I have extracted these questions and answers from my own notes, which are more full than the minutes of the Board, in which the words *underscored* are omitted.

The witness then underwent a medical examination upon the nature and properties of fevers, upon which it would be presumptuous in me to offer any further remark than that the impression produced by it upon my mind was, that the witness was a man of remarkably clear head and acute understanding, possessing (as indeed was to be expected from his great experience) intimate knowledge of his subject.

No doubt having been suggested as to the skill or veracity of Mr. Amiel, and no attempt, not even an insinuation, having been made to impugn or question his evidence concerning these five cases, I cannot but receive that evidence as conclusive.

Dr. Browne, assistant surgeon of the 23rd Royal Welsh Fusiliers, stated that in May 1826 he saw two cases of fever in the Civil Hospital, which he considers to have been identical with the *more severe cases* of the late epidemic; that these were the cases of two Jews, the survivor of whom enjoyed during the late epidemic the usual immunity from a second attack.

Mr. Gillice, assistant surgeon of the 12th Regiment, gives evidence to the same effect as to the cases of the two Jews; and he adds another case, that of Lieutenant Ottly, of the 23rd Regiment, who was attended by Dr. Smith, a gentleman who was absent from Gibraltar at the time the Board was sitting.

Dr. Gillkrest, surgeon of the 43rd Light Infantry, speaks to two sporadic cases in the year 1824; one that of private John Pierson, of that corps, whom he attended until, in the course of such attendance, he himself was taken ill; his own case is the second. Both of these cases he identifies with the epidemic. He was attended by Dr. Arejula, physician to the king of Spain, who considered this to be a sporadic case of the Yellow Fever of Andalusia.

Staff-surgeon Dix only saw 10 or 12 cases during the epidemic, he having been absent, and not having returned to Gibraltar until its close. He states that those cases appeared to him to differ in no respect from the bad Sporadic Yellow

Fever eases which he had witnessed annually during his residence on the Rock from 1820 to 1826; two or three times between 1820 and 1826 he saw at the Civil Hospital eases attended with the black vomit.

These are the only military medical officers who speak to the occurrence of sporadic eases of the disease in seasons not epidemic. With regard to this class of witnesses, it is to be remarked that the smallness of their number, and the limited period of time which most of them speak to, are probably attributable to the frequent changes of station which characterise the military service.

Messrs. Fraser and Wilson, the medical officers attached to the Civil Hospital, and both having extensive private practice, deposed to their having annually witnessed eases of fever identical with the epidemic. Each of these gentlemen underwent a long controversial cross-examination touching the symptoms, characters, and qualities of fevers, continued, remittent, and intermittent, their causes and effects, and typhus icteroides, and the typhus of Ireland, and agues and malaria, and sundry speculative opinions touching all these matters, of which disputation I am unable fully to appreciate the merits, excepting in so far as it shows that the two gentlemen of the faculty who were under examination are men of considerable ability, who had (as was their especial duty) closely studied and investigated the fevers of Gibraltar, who have professionally been much engaged in the treatment of those fevers in the public hospital of which they have the medical charge, and whose opinions, consequently, are entitled to the most respectful attention.

Mr. Wilson declares that he treated about 200 cases of the late epidemic; that he had, between the years 1811 and 1813, seen the same disease (*i. e.*, the Black Vomit Fever of the West Indies) in Jamaica, in Carthagena (South America), and in the Havannah; and that he had *in the Civil Hospital at Gibraltar*, generally every year, from 1815 to 1828, met with cases of the Black Vomit Fever of the West Indies, accompanied by the same symptoms, and terminating in some patients by death under the same circumstances as eases of the late epidemic. He added that he had carried from the West Indies to England a specimen of the Black Vomit, and speaks positively to the identity, with it, of the Black Vomit which he had observed in sporadic cases at Gibraltar between 1815 and 1828.

Mr. Fraser stated that he had treated about 200 cases of the late epidemic, and that he had seen many, perhaps 500 or 600 more; that he had been surgeon to the Civil Hospital about five years, which situation necessarily forced him to pay attention to the general health of the place, and that he had directed his attention particularly to the subject of fever; that during the five years of his residence, and previous to August

1828 (when the epidemic broke out) he saw between 40 and 50 cases which he is inclined to identify with the epidemic; that he saw no difference in symptoms between these sporadic and the epidemic cases, even those with the black vomit; that the mortality in these sporadic cases was great, perhaps 1 in  $3\frac{1}{2}$  or 4; that he has no doubt as to occurrence of black vomit in some of these cases similar to the black vomit observed in the epidemic, nor has he been able to distinguish any difference in the post-mortem appearances observed on the dissections of several of the bodies of those who died previous to and of those who died during the epidemic: he added that an analysis of the black vomit had been made in the Civil Hospital long before the breaking out of the epidemic.

The whole of Mr. Fraser's and Mr. Wilson's examinations, on the different occasions when they appeared before the Board, are extremely material, nor can they be abridged without rendering them obscure.

Mr. Fraser, in support of his opinion that the disease was indigenous, presented to the Board a list of 39 cases of Yellow Fever, recorded in the case books of the Civil Hospital as having occurred in years when no epidemic prevailed. To counteract this, evidence was adduced to show that some few of the patients in these sporadic cases had suffered the fever in some epidemic season, whence it was intended that we should infer that the cases presented by Mr. Fraser must have been cases of some other disease.

Had this been satisfactorily proved, the credit of the case-books and the testimony of Messrs. Fraser, Wilson, Dix, Browne, and Gillice, might have been in some measure shaken, inasmuch as it had been decided by the Report of a Medical Commission that "one attack of Yellow Fever preserves the individual "from a second."

In opposition, then, to Mr. Fraser's list, the hospital patients themselves in some instances came to depose to their having had the Yellow Fever in some epidemic year; and in other instances, when the patient himself was dead, or not to be found, his relatives, or some one who said he knew him, gave evidence to the same effect. No *medical* evidence whatever was produced; but the bare assertion of a bricklayer, a labourer, or a journeyman butcher (this was the occupation of Sabah, as deposed by himself, though it does not appear upon the minutes), was given as evidence capable of outweighing the professional opinions of the medical attendants of the hospital supported by the entries in the case-books. On a professional point such impeaching evidence would be worthless under any circumstances; but on this particular occasion the impropriety of receiving it as conclusive is aggravated by a remarkable instance of inconsistency.

The Report of the Commission, which is made the test of the

hospital cases, contains the following sentence:—"Four physicians had attended in 1828 some patients who said that they had had the Yellow Fever in one or other of the former epidemics; but as the symptoms of the first attacks could not be stated, the Board could not take these cases into consideration." Now as it is certain that no symptoms were stated to the Board by the journeyman butcher or his colleagues in their attempt to shake the credit of the hospital cases, it would seem to be particularly unfair to admit, as sufficient to overturn these cases, such evidence as would have been rejected by the Commission whose Report is made the foundation of this line of inquiry.

The same evidence, to establish the same fact, cannot be *good* in one place, and *bad* in another. If *good* against Mr. Fraser's cases, the Report of the Commission, which excluded such evidence as *bad*, must of course be itself worthless, as being founded on insufficient testimony, and then the fact found by that Report cannot be assumed as a test to try any question whatever; but if such evidence be *bad*, and the Medical Commission were right in rejecting the loose assertions of ignorant men, unsupported by any specification of symptoms, or by the testimony of a medical attendant, then the evidence of Sabah and the rest must be rejected by us, and the genuineness of the civil hospital sporadic cases stands unimpeached.\*

A long medical discussion took place upon some three or four cases quoted by the late Dr. Hennen, in his Annual Report for 1827; and it was disputed whether the patients named therein had the Yellow Fever, and whether they contracted their disease in Gibraltar or in the adjoining country; upon which I am incompetent to form an opinion, nor does it appear to me to be very directly material to the question as to the origin of the epidemic; and I accordingly dismiss these cases altogether from further consideration.

Divers medical men, chiefly the civil practitioners, deposed to their not having met with any sporadic cases in the course of their practice: this evidence might well have been spared, because it amounts to nothing; for witnesses are called to declare what they *do* know, not what they do *not* know; what they *have* seen, and not what they have *not* seen.

The occurrence of sporadic cases of this disease in years not epidemic is confirmed by the general course of events at the beginning of the late epidemic. The alleged first cases (those of Fenic's children) struck their medical attendant with no surprise or alarm: if these cases had presented appearances which he had not been accustomed occasionally to see—even what is described as "the fatal symptom of black vomit"—he would

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\* Vide the Certificate, Dr. Gillkrest's Report, p. 155—of the existence of which I was not aware when this paper was written. T. J. H., 7th March, 1851.

naturally have been astonished and alarmed at the occurrence of a disease foreign to his practice. In the earliest cases, no medical man who had not witnessed an epidemic denounced the appearance of an unknown disease unusually rapid in its progress, or marked by any unusual symptoms, nor did any medical man who had witnessed an epidemic suddenly proclaim to the community his having for the first time since the year 1814 seen a case identical with that disease, and warn the public of the recurrence of a malady from which they had been for fourteen years exempt. On the contrary, judging of men's real opinions by the safest criterion,—their own acts,—I find that the circumstance which caused experienced as well as inexperienced medical men to announce the presence of the Yellow Fever epidemic was not the occurrence of one, of two, or of more cases, but the gradually increasing number of cases of the same disease, from day to day, in the same district of the town;—that it was not by *anything unusual in the nature of the disease*, but by *something unusual in the number of cases*, that the advent of a Yellow Fever epidemic was ascertained.

Several medical witnesses were examined as to their opinion of the origin of the epidemic. Of these the witness who has had the most extensive experience of the disease is Dr. Chervin, one of the members of the French Medical Commission.

This gentleman appears to have made the Yellow Fever his especial study, and to have prosecuted his inquiries in every part of the world where it is to be found. He had witnessed the disease in Guadaloupe; in the city of San Domingo, and in Port au Prince, in the same island; in Kingston, Up Park Camp, and Port Royal, Jamaica; in St. Jago de Cuba; in the city of Havannah; in New Orleans; in Savanna; in New York; in Martinique; in Barbados; in Demerara; in Barcelona; and in Gibraltar during the last epidemic; and he had personally investigated the localities of several towns in the south of Spain in which Yellow Fever epidemics had existed; and of which he narrates some very remarkable circumstances. He pronounces a very decided opinion, supported by reasoning, that the epidemic originated in Gibraltar. I may here remark, that, in the course of his evidence as given in the minutes, there is an omission which strikes me to be of some moment. In his answer to Question 14, Dr. Chervin stated that he had dissected subjects that had died of remittent fevers, "*and in those subjects I have found the same appearances as in Yellow Fever.*" These words I supply from my own notes.

Mr. Dow, staff surgeon, who had witnessed the epidemic of 1813 in Gibraltar, and who had seen the same epidemic disease in the West Indies, is of the same opinion, that the disease is indigenous.

Dr. Gillkrest, surgeon of the 43rd Light Infantry, declared the same opinion.

I forbear to enlarge upon the long and argumentative evidences of Mr. Wilson, (who had resided in Gibraltar from 1815, and who had previously witnessed the same disease in Jamaica, in South America, and at the Havannah,) of Mr. Fraser, and of Mr. Amiel, who had witnessed all the epidemics of Gibraltar from the year 1810; all of whom decidedly attribute the epidemic to local causes.

Mr. Dix, staff surgeon, also attributes the origin of the epidemic to Malaria.

A very learned anatomical dissertation was addressed to the Board by Dr. Louis, one of the French Medical Commission; in the course of which he pointed out the anatomical appearances observed on the dissection of subjects that had died of fevers in Paris; he also described the appearances of subjects which he had dissected at Gibraltar, and which had died of the epidemic Yellow Fever. Dr. Louis's experience, however extensive in regard to the number of subjects, appears to have been confined to Paris; and, indeed, he candidly acknowledged that, not having visited any hot climates, he was practically unacquainted with the diseases incident to them.

Dr. Trousseau, the third member of the French Medical Commission, gives evidence much to the same effect as Dr. Louis. His experience had been confined to France.

In the course of the evidence of these two gentlemen, which was given with every appearance of skill as well as of truth, neither of them gave any opinion as to the origin of the disease; and however valuable their anatomical observations may be, in a professional point of view; so far as the inquiries of the Board were concerned, their evidence appeared to me to amount only to this—that they had never seen the Yellow Fever in France, and that they had seen it at Gibraltar.

Of the other medical men, several declined to commit themselves to any positive opinion.

Many, indeed, and these chiefly the foreign civil practitioners of Gibraltar, give very decided opinions that the disease was imported. Without wishing to deal illiberally towards the foreign practitioners on the Rock, I cannot but observe that they are persons whose information and habits are not such as to entitle their opinions and assertions to that respectful consideration which the testimony of scientific men, upon scientific subjects, ought to command. The extreme hardihood with which assertions are made by this class of witnesses, who cannot possibly know their truth, is instanced in the case of Dr. Zela, who having been asked, "Is this opinion founded upon *observations made by yourself*, and, if so, state them?" he replied, "it is so founded," and then proceeded to narrate the circumstances; he was then

asked, "whether these circumstances had fallen under his own personal observation? whether he had derived them from books or heard them from other persons?" he then declares "the circumstances I have mentioned are stated in medical dissertations on the disease." Of a like character is the account of the propagation of the Yellow Fever on board a smuggling vessel, given by Dr. Cortes, the owner of that vessel—an account upon the face of it extremely improbable, which he could not know to be true, and which he could only have derived by hearsay from persons engaged in an occupation which is certainly not characterized by a scrupulous adherence to truth.

Such also is the evidence of Dr. Ardevol, who describes himself as having practised "publicly and privately" six years and a half, who states, "I consider this disease as contagious; not only am I of opinion that this disease can be transported from one place to another, but in Spain it is known as an historical fact, that this disease *has been transported on horseback* from one village to another. In the history of the epidemic of Los Barrios, you will find the *name* of the cavalry soldier who brought the disease from Cadiz to that town."

Of a similarly vague and inconclusive character are the evidence and reasoning of the other witnesses of this class such as Dr. Martinez and the rest, to enter upon an analysis of which would be a mere waste of time. I have thought it just to make these remarks because these witnesses were not subjected to any professional cross-examination, as was the case with the medical witnesses on the opposite side of the question.

That the drains of Gibraltar emitted most foul exhalations is proved by evidence perfectly incontestable. I refer to the testimony given not merely by the medical gentlemen, but by Mr. Woodward, surveyor of revenue buildings, by Lieut.-Colonel Bayley, by Major Middleton, by Colonel Pearson, by Captain Crawford, by Quarter-Master O'Grady, and by others; that these exhalations were such as not only to pervade most houses, but universally to corrupt the atmosphere, is proved by Major-General Pilkington, the Commanding Royal Engineer.

That the population was redundant, and that the poorer classes were crowded together in dirty ill-ventilated abodes, is shown by the letters of the late Dr. Hennen (who expressly pointed to these on 29th August 1828, speaking of No. 24 District, as sources of disease), and by the testimony of Mr. White, collector of Her Majesty's revenues, of Mr. Woodward, of Colonel Pearson, &c.

That the poverty and distress of the lower orders had been progressively increasing up to the commencement of the epidemic is proved by the evidence of Mr. White and of Mr. Woodward, which is corroborated by General Pilkington's declaration that the demand for labour had decreased, while

it is proved that water is so scarce as to be retailed as an article of purchase by the keg.

Reviewing the whole of the evidence, then, I find that the asserted importation of the disease rests chiefly upon the *scientific* opinions of the Spanish practitioners, and that the facts to support it come from the mouths of washerwomen, cigar-makers, and females of the same class as Mary Galt and Mary Parody; while the proof that the epidemic had its origin in Gibraltar rests upon the opinion of Dr. Chervin, who has probably seen more of the disease than any other man in existence, of the surgeons of the public hospital, whose duties led them to an intimate knowledge of the diseases of the climate and the place, and of the English medical officers; and that the facts to support this doctrine are proved by the commanding and other officers of regiments, by the chief engineer, and by several officers of the civil service, whose characters and station in society are guarantees for the truth of their assertions.

London, 9th Nov. 1829.

(Signed) T. JONES HOWELL.

#### SUPPLEMENTARY NOTE.

On a re-perusal of the foregoing Report, I see no reason to doubt the soundness of the conclusions which a very careful and minute analysis of the evidence forced upon my mind; and my conviction remains unshaken, that the testimony of the child Caffero, and of the other witnesses who were associated with him in the bungling endeavour to prove such a case as would establish the doctrine that yellow fever is contagious, and that the Gibraltar epidemic was an imported and not an indigenous disease, was not only false but suborned—(*Ante*, p. 250.)

It was my misfortune to differ from the medical members of the Board of Inquiry, viz., Dr. Pym, superintendent-general of quarantine, Dr. Broadfoot, medical officer of quarantine at Gibraltar, and Staff-surgeon Barry; but not having subsequently lost sight of questions to which my attention was on that occasion called for the first time, it has been some consolation to me to observe, that opinions, which appeared to me to be as true as they were then unpopular with persons in authority, have been steadily advancing with advancing knowledge; that the doctrines upon which the quarantine system was founded are becoming gradually exploded, and that the attention of mankind is now with greater wisdom directed to the extirpation of those inbred causes of pestilence which grew and flourished under the mistaken policy that inculcated the reliance on quarantine and lazarettos as safeguards against the invasion of epidemic diseases.\*

The coincidence is remarkable that those countries, such as Spain, of which the commercial policy is based on high duties, prohibitions, and monopolies, and in which, consequently, smuggling and bribery are most active, are also those in which the preservation of the public health is based on the doctrine of contagion, and on the infallibility of quarantine.

But whatever efficacy may be attributed to the restrictions of quarantine as they affect the undisguised intercourse of legitimate commerce, it is clear that they apply no remedy to the clandestine operations of the smuggler, while it is equally evident that the more you appear to make your quarantine restrictions effectual by multiplying their number and increasing their stringency, the more do you in reality encourage the contraband intercourse of the smuggler, who, by bribery, fraud, or force, sets all your restrictions at naught.

The intercourse which is constantly kept up by the overland route between Europe and Asia through Cairo and Alexandria would seem to present favourable opportunities for disseminating the plague, if that be a disease capable of transit by contagion.

March 7, 1851.

T. JONES HOWELL.

\* "And he who were pleasantly disposed could not well avoid to liken it to the exploit of that gallant man who thought to pound up the crows by shutting his park gate."—*Milton's Arcopagitica*.

EXTRACTS from OFFICIAL CORRESPONDENCE relative to the PROCEEDINGS of the BOARD of INQUIRY as to the ORIGIN of the YELLOW FEVER EPIDEMIC at GIBRALTAR in 1828.

AFTER the proceedings of the Board had terminated to which each member had appended his opinion,\* a detailed report was addressed by Dr. Pym to Sir George Don, and transmitted [by him with those opinions for the information of Secretary Sir George Murray. Subsequently the Minutes of the Board were also forwarded, on the receipt of which, the Secretary of State addressed a Despatch to the Governor of Gibraltar, dated 2 July 1829, from which the following is an extract :—

“ It is with regret I find that the question as to the origin of the fever, which for all immediate purposes of Government I was most anxious to have thoroughly investigated, and cleared from doubt, remains unsolved; for, placing out of view the qualified opinion of Dr. Broadfoot, the vote of the Town Major, and the grounds upon which the Captain of the Port declined giving any opinion,† I observe that, while Drs. Pym and Barry have, on the one hand, very decidedly given their opinion, and supported it by reasoning which is entitled to the most serious attention, that the fever was imported; two other members of the Board have in an equally decided manner, denied that, in their opinion, any proof was produced to warrant the belief that the fever was imported. And as much stress has been laid by one of these members, Colonel Chapman, upon the description of evidence which was brought forward, and the manner in which it was given, I think it important that he should furnish me with an explanation of the reasons which he had for thus expressing an opinion which goes to discredit the whole proceedings of the Board,”

In reply to this request of Sir George Murray, Colonel Chapman‡ wrote to Sir George Don, a letter dated Gibraltar, 9th August 1829, which contained the following passages :—

“ Generally speaking, the witnesses brought forward in support of the hypothesis, that the late epidemic disease was imported, were of the lowest class; they had all, as I understood, undergone private examinations, which were but in a few instances submitted to the knowledge of the Board; and much of this carries strong marks of previous preparation for the

\* See Appendix, No. I., page 161. † See Mr. Sweetland's opinion, p. 214.

‡ Lieut.-General Sir S. R. Chapman, C.B., K.C.H., late Governor of Bermuda, and since deceased.

public examination. \* \* \* I have alluded to private examinations \* \* \* Mr. Howell, the Judge, and Mr. Sweetland, the Captain of the Port, as well as myself, did, on several occasions, express their disapprobation of this practice: the latter, I well remember, frequently so; and when his objections were overruled, he made a motion,—which was also lost,—that all information touching the matter at issue which should be collected by any member of the Board, should also be communicated to the Board previously to the examination of the witnesses on it, in order to the proper management of such information, and to put all the members of the Board on an equality. That private examinations were taken by some of the members of the Board is certain; that they had much influence on the minds of those who have either taken or received them, is scarcely less so; and I am by no means free from doubt, that such had some influence in the composition of the report [by Dr. Pym] of the proceedings, spoken of in Sir George Murray's Despatch. It was on this ground that I, and probably the other members of the Board,—forming the majority on that occasion,—objected to the production of it; it was set aside; forms no part of the proceedings; and I am ignorant of its contents."

On the 18th November 1829, Sir George Murray addressed to Sir George Don a despatch, requiring to be furnished with no less than about 24 documents referred to in various parts of the Minutes of the Board of Inquiry, but which had been altogether omitted on the face of them; and amongst others, "that which is most highly material, the Bill of Health of the Swedish ship 'Dygden.'"

This despatch contains the following description of the state in which these Minutes were presented:—

"The communications and evidence upon the same subject are so widely detached, and with no note of reference from one to the other, that it is all but impossible to pursue connectedly any one question agitated on the proceedings. \* \* \* Very many errors purely clerical, appear on the face of the Minutes; but some are such as materially to alter the effect of the testimony."

## APPENDIX No. III.

## REMARKS ON WEST INDIAN FEVER, &amp;c.

By Dr. A. BROWNE.

*London, 3rd January 1851.*

MY LORDS AND GENTLEMEN,

IN reply to the request transmitted to me from your Honourable Board, I beg to say that any observations of mine on the subject of Yellow Fever, which may be deemed useful, are entirely at the service of the public; and I now venture to submit for your consideration, the following notes on fever in the West Indies and coast of Africa, founded chiefly on facts and observations contained in the statistical reports for the army from 1817 to 1836, and for the navy from 1837 to 1843.

THE MORTALITY from fever among the white troops serving in Jamaica, on the average of 20 years, has been in the following proportions to 1,000 of mean strength, at the undermentioned stations, viz.:—

Montego Bay	.	.	.	150·7
Spanish Town	.	.	.	141·1
Port Antonio	.	.	.	126·0
Up Park Camp	.	.	.	121·0
Port Royal	.	.	.	93·9
Falmouth	.	.	.	80·0
Stoney Hill	.	.	.	70·5
Lucea	.	.	.	63·2
Fort Augusta	.	.	.	55·5
Maroon Town	.	.	.	15·3

*Vide Table LX., p. 70.*

N.B.—The mortality from fever among the black troops, on an average, has been less than 1 per cent., and may be disregarded, as the strength of these troops at the several stations is not known.

At some of the small stations, temporarily occupied during disturbed periods, the mortality has far exceeded that at any of

the permanent posts here given; but as the time they were held was short, and the force small, they are not entered here.—*Vide* Report, p. 69.

TABLE of FEVERS for Jamaica, p. 46, Report.

—	Admissions.	Deaths.	Proportion of Deaths to Admissions.
Intermittent Fever . . . . .	6,090	37	1 in 165
Remittent . . . . .	38,393	5,114	1 in 8
Common continued . . . . .	1,971	86	1 in 23
Yellow Fever (Icterodes) . . . . .	20	15	1 in 1 $\frac{1}{3}$
Synochus . . . . .	448	1	1 in 448
Total . . . . .	46,922	5,253	1 in 9
Annual rates per 1000 of Mean Strength .	910	101.9	. . .
Average Strength . . . . .	2,578		
„ Admissions . . . . .	4,672		
„ Deaths . . . . .	313		
} from all Diseases.			
Total Strength in 20 years . . . . .	51,567		
Ratio of all Admissions per 1000 . . . . .	1,812		
„ Deaths per 1000 . . . . .	121.3		

NOTE.—The 20 cases of Yellow Fever in this Table occurred in a detachment at Lacovia, on the Black River, a post about 12 miles from the sea, in 1834, when no epidemic Yellow Fever existed in any other part of Jamaica: 15 of those patients died in Fort Augusta; nevertheless, the deaths from all causes, at the latter station, were in that year under the annual average from fever alone.—*Vide* Report, pp. 47 and 54.

Fevers, “though only in a small degree more prevalent than in the Windward and Leeward command,” are “nearly thrice as productive of mortality, in consequence of about three-fourths of the cases being of a remittent type, which is extremely fatal; whereas, in the other command, little more than a fourth of the cases are of that description.” The mortality from the remittent type, however, has been 1 in 8 in both commands on the average of the whole 20 years; this type is therefore not more severe, but only more prevalent in Jamaica.

The influence of the seasons on the prevalence of fevers in Jamaica cannot be ascertained in a separate form, from the tables in the Report, as the statement at p. 46 from the Medical Quarterly Returns relates only to four epidemic years; but even this statement proves that the mortality in the second six months of these years has been to that of the first six months, in the proportion of 1,914 to 692, or nearly 3 to 1.

In the General Table of Admissions and Deaths by Months at p. 71, for a period of 18 years, the admissions during the first six months were 27,766, and in the second six months 33,335, by all acute diseases. And the deaths from acute diseases were 1,751 in the first period, and 3,569 in the second. The admis-

sions therefore in the first half of the year, were to those in second half, as 1 to 1·2, and the deaths as 1 to 2·03.

It may perhaps be objected to this mode of testing the influence of season on the prevalence of fever, that bowel complaints or other affections, which acknowledge the influence of the autumnal season, caused this increase in the admissions and mortality; but this objection is only specious, as bowel complaints form only one-fifth or thereabouts of the admissions, and caused about a twenty-fourth part of the total mortality; while fever, on the contrary, occasioned three-fourths of the former, and five-sixths of the total fatality. If the influence of season has thus been less marked in increasing the febrile cases, in so far as concerns their number, there can be no doubt of its having impressed them with a malignancy, which is only the more remarkable from its occurrence within the tropics, where the variations of temperature, according to the season, are by no means great.

It may be important to ascertain what effect this influx of fever patients had upon those with other diseases in the hospitals.\* The Table referred to shows that in 10,258 surgical cases, the rate of mortality, in the first six months, was 3·4 per 1,000, while in 8,639 the rate was reduced, in the next six months, to 2·4 per 1,000. On the contrary, in 3,628 chronic cases, 79 per 1,000 died in the former, whereas in 2,915 cases, 104 per 1,000 died in the latter period. These results are contradictory, and we may perhaps assume, for the present, that the fever patients exerted no very deleterious influence on their comrades in the hospitals; for the more numerous admissions, in the earlier half of the year, of cases, which are slow in their progress, would lead us to expect a larger mortality at a subsequent period.

The immense differences in the number of fever cases, and in their proportional mortality, in certain localities, differing little in climate, distance, or surface, from others, might convince the most incredulous that the cause of this disease is of local origin, or, at all events, local in its action; otherwise the mean mortality from fever would not amount to 104·1 per 1,000 in Tobago, and fall to 11·2 at St. Vincents; or rise to 63·1 per 1,000 in St. Lucia, and sink to 11·8 at Barbadoes; or reach 61·6 per 1,000 in Trinidad, and only 14·9 in Antigua; during the long period of 20 years comprised in these Reports.

On the supposition that the great epidemics are of foreign origin, being imported and contagious, it may be said that the foregoing examples of its local severity are of no moment, as the disease might be introduced into one island without extending to others. In order to determine the value of this objection, we have only to refer to the Table (60) for Jamaica, where we find that at Spanish Town the mean mortality from fever has

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\* For effects on *Orderlies*, see Bancroft's Sequel, &c., p. 186.

been 141·1 per 1,000, while at Fort Augusta it was 55·5; at Up Park Camp 121·0 per 1,000, and at Stoney Hill 70·5; again, at Montego Bay, 150·7 per 1,000, and at Maroon Town 15·3; and lastly, at Port Antonio, on the north side, 126 per 1,000, and at Port Royal, on the south, 93·9; these numbers being, in all cases, the average deaths for long periods, and in the majority for 20 years.

In all the latter instances, with the exception of Port Antonio, and Port Royal, the stations are within short distances, the communications are frequent between them, and any contagious disease prevailing at the one might be easily transmitted to the other; and Port Antonio was contrasted with Port Royal, because the former is a small port where few ships, unless from England or North America, ever enter; and Port Royal, on the contrary, is more frequently visited by vessels from every quarter than perhaps all the other harbours in Jamaica.

If we corroborate the preceding proofs of the localization of fever, by the facts so clearly established by this excellent statistical Report; viz., That epidemic fever is rarely if ever absent from all the islands in the Windward command, and all the stations in Jamaica, though it has never afflicted them all in any one year, notwithstanding the communications were not interrupted:—and that the removal of sick or troops from one post affected, to another exempt from its influence, has so frequently arrested the epidemic, without endangering others, that removal has become the rule, isolation the exception:—The conclusion that the disease which decimates our troops in the West Indies is in its origin truly endemic, appears inevitable; and further, that whenever it assumes an epidemic form, it is not in any way changed in character, or possessed of new or adventitious qualities of propagating itself, however much it may differ in severity, because removal is still effectual in checking its progress to the same extent, provided the new locality be sufficiently elevated or healthy,—as happens always when troops are removed from Montego Bay to Maroon Town.\*

Table LXI. (at p. 70) shows that the mortality, at the 10 principal military stations in Jamaica, has exceeded the mean rate of each station by one-fifth, in the following years, viz.:—

1817, Port Antonio.

1818, Spanish Town.

1819, Up Park Camp; Port Royal; Fort Augusta; Stoney Hill; Port Antonio; Falmouth.

1820, Spanish Town; Falmouth.

1821, Port Royal; Port Antonio.

1822, Up Park Camp; Spanish Town; Port Royal.

1823, Spanish Town.

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\* The two posts of Vivario and Vitzavona, on the high central ridge of Corsica enjoy the same exemption from the fatal fevers of Saint Florent and the coast, which Maroon Town does from those of the low-lands of Jamaica.

1824, Spanish Town; Port Antonio; Montego Bay.

1825, Up Park Camp; Port Royal; Spanish Town; Stoney Hill; Falmouth; Lucea.

1826, Falmouth.

1827, Up Park Camp; Port Royal; Fort Augusta; Stoney Hill; Falmouth.

1828, Montego Bay; Maroon Town.

1829, Port Antonio.

1830, Port Royal; Lucea; Maroon Town.

1831, Spanish Town; Stoney Hill; Falmouth; Maroon Town.

1832, Port Antonio; Montego Bay; Lucea; Maroon Town.

1833, Port Antonio; Maroon Town.

1834, None.

1835, Spanish Town.

1836, Montego Bay; Lucea.

As fever causes five-sixths of the average annual mortality among the troops in Jamaica, it has been assumed that whenever the total mortality at any station exceeded the mean increased by one-fifth; it arose from the prevalence of fever in an epidemic form at that station.

Should this be admitted (and it appears to me that it can hardly be denied), it follows that epidemic fever during the 20 years comprehended in the Report, was for 6 of those years limited to one of 10 stations.

For 5 years limited to two stations.

For 3 years limited to three.

For 2 years limited to four.

For 1 year limited to five.

For 2 years limited to six stations; and for 1 year it did not prevail as an epidemic at any one of the 10 principal stations.

It would appear from the preceding detail of stations that no constant relation exists between their proximity, or the frequency of intercourse between them, and the prevalence of fever; the only exceptions being in years of wide spreading epidemics when several suffered in the same year, though frequently not at the same time; thus during the terrible epidemic of 1819 at Up Park Camp, though several detachments of the 50th, and 92nd Regiments, were sent to Stoney Hill between July and October, yet fever did not appear at the latter station until October, and then the mortality was less than one-third, whereas at Up Park Camp it had amounted to half the force. The Report, however, says, "It is rather remarkable that epidemics generally break out here (Stoney Hill) at a different period from that at which they make their appearance in the low grounds."

Detachments were also sent from Up Park Camp to Fort Augusta, but at the latter, generally healthy station, the mor-

tality did not exceed 1 in 10 of the strength. It is, moreover, not a little singular, that in Spanish Town, surrounded by this fatal disease, the deaths were less than half its annual average by fever. In the following year, however, Spanish Town lost more than a fourth of its garrison, and kept up its character, "that fever seldom appeared in the island, without showing itself there in a very aggravated form, and was exceedingly prevalent and fatal even at times when all the other stations were comparatively healthy."

Indeed, from 1822 to 1826 inclusive, this station suffered annually from epidemic fever,—while Stoney Hill, situated in a mountain gorge at no great distance, suffered in 1825 only, among these five years. Port Royal, on the east side of the harbour of that name, suffered heavily from fever in 1819, 21, 22, 25, and 27; while Fort Augusta, four miles distant on the opposite side, suffered considerably in 1819 and 1820, and terribly in 1827, but not in any other year. On the north side of the island, the epidemics of Port Antonio occurred in 1817, 19, 21, 24, 29, and 33; of Falmouth, in 1819, 20, 25, 26, 27, and 31; of Lucea, in 1825, 30, 32, and 36; Montego Bay, between Lucea and Falmouth, and constantly communicating with both, had its epidemics in 1824, 28, 32, and 36; in the two last years, Lucea also suffered much; but Falmouth, 16 miles to the east, on the highway to the capital, and indeed on the line of greatest intercourse, was very healthy, particularly in the latter year. It appears necessary to add, that "Kingston," the garrison of which had for some time been furnished from the force in Fort Augusta, "is one of the most unhealthy quarters in the island; and a very great portion indeed of the deaths recorded in the above table (p. 55) originated there; we have been able to trace, and deduct not fewer than 56 of these in the year 1825 alone, but in none of the other years could we effect any accurate separation, and can therefore only recommend it to be kept in view, that the two companies at Kingston always furnished more fatal cases than the four which were at Fort Augusta, and with the proper correction on that principle, the mortality cannot have exceeded 5 per cent. annually. Though its locality is apparently so unfavourable," whilst it was thus undergoing an annual importation of fever, Fort Augusta presents a lower mean rate of mortality than any of the other coast stations in Jamaica, during the long period of 20 years.

If we turn to Maroon Town, an elevated and healthy post in the interior, we find that "during several years in which troops were not permanently quartered at Montego Bay, it was customary to send detachments there from Maroon Town during the negro holidays, and these always brought back a large proportion of sick and many fatal cases. The detachments at Falmouth and Lucea too, when sickly, have occasionally been

relieved by healthier ones from this post, and it has sometimes happened that the corps sent to Maroon Town had previously been suffering under a great mortality in other parts of the island, and brought with them many sick in a dying state. After a diligent investigation, it appears that from 30 to 40 of the deaths included in the above table (p. 64), may fairly be attributed to one or other of these causes, so that the actual mortality of the station has not exceeded 22 per 1000 of the force annually."

I may here state that the annual mortality from fever forms only about one-half of the total mean mortality at this station, consequently the influx of fever patients cannot be said to have introduced contagion among the troops quartered there.

In the stations of the Windward command, the mortality from fevers bears so variable a proportion to that from other diseases, in the different islands, as to cause less than half the deaths, on the average, in the whole command, and the application of the same method to determine their epidemic prevalence, would be liable to serious objection.

Besides the frequency of intercourse between the several islands being uncertain, it would never afford the same probability of the extension of the disease by contagion, as in the same island where communication is always active by means of good roads.

In the various islands, as in Jamaica, posts at one time occupied by white troops have, of late years, been abandoned as unhealthy. Without entering, however, into such details, the following statements, taken from the Appendix to the Report, will show that these colonies have presented the greatest differences in the frequency, and fatality of their fever epidemics. From the abstract, No. 2, p. 5, of the Appendix, it appears that fever was prevalent and fatal in British Guiana in 1819, 20, 21, 24, 25, 26, 27, 28, 30, and 31; but *Yellow Fever* is mentioned only in 1820, 21, and 25. In the first of the latter three years, more than half; in the second, one-seventh; and in the third, about a fourth of the deaths from fever were attributed to the yellow variety of this disease. The mortality which was about 16 per cent. in 1820, was less than 14 per cent. in 1821, and only 6.6 per cent. of the white force in 1825. In 1827 and 1828, when this variety was not recorded, the loss was 12 and 11 per cent. of the strength respectively.

In 1818, 30 per cent. of the white troops in Trinidad died of fever, and in 1828, the rate of mortality was 13 per cent., while in the six years, from 1820 to 1825, the average was only 3.2 per cent. of the force; but *Yellow Fever* is not mentioned in this abstract.

By the return at p. 8, for Tobago, it appears that in 1818, the mortality was 13 per cent.; in 1819, it was 18 per cent.; in 1820, it was 80 per cent.; and in 1821, it amounted to 25 per

cent. of the white force. In these four successive years, about four-fifths of the cases were called Yellow Fever; while, in 1817 and 1830, when this variety was *not* mentioned, about 14 and 12 per cent. of the troops died of fever. In five years (of the 20) the rate of mortality from fever was under 2 per cent.

In the abstract, at p. 9, for Grenada, Yellow Fever appears twice,—namely, in 1817, when 8·2 per cent.,—and in 1818, when 2·1 per cent. died of fever; but in 1819 and in 1828, it was *not* mentioned, though 5·7, and 9 per cent. of the force, were cut off in these years.

The abstract No. 7, for St. Vincent's, shows that the average annual mortality from fever was 1·12 per cent. of the strength; and in 1822, when *one* yellow case appeared, the rate increased to 2·1 per cent.; but in 1824 and 1825, the mortality was twice as high, though *no such case* has been recorded. The return, No. 8, for Barbadoes, shows that fever prevailed there in 1817, 20, 21, and 22. In the first of these four years, the mortality was 3·3 per cent. from all fevers, Yellow included; in the second, 5 per cent., chiefly from Yellow Fever; in the third, 7·4 per cent., one-third of the fatal cases being Yellow Fever; and in the fourth, 3·3 per cent. with no Yellow Fever; whereas, in 1818, 19, and 27, the rate of mortality was under 1 per cent., with *one or two* cases of Yellow Fever in each of these three years. We learn from the abstract, p. 13, that fever was prevalent and fatal in St. Lucia in 1818, 22, 24, and 27. In the first of these years the mortality was 14·5 per cent.; in the second, 30·8 per cent.; and in the third and fourth, 21 per cent. of the white force; while only *one* case of Yellow Fever is recorded in the second, and *one* in the fourth, and *none* in either of the other years.

Dominica appears from the abstract, p. 14, to have lost 29 per cent. of its white garrison from fever in 1817; 36 per cent. in 1821; and about 9 per cent. in 1826; though Yellow Fever is *never* mentioned. The abstract, at p. 15, for Antigua and Montserrat, proves that, with the exception of 1835, fever had not prevailed epidemically within the 20 years, from 1817 to 1836, as the mortality from that disease rarely exceeded 2 per cent. of the strength; notwithstanding, 1 *in every* 9 fatal cases was Yellow Fever, and, moreover, 35 sailors died in the military hospitals from this disease, in the single year 1822, in which the troops lost only 1·7 per cent. of the strength from that, and other fevers.

St. Kitts, Tortola, and Nevis, appear to have suffered pretty severely from fever in 1818, 20, 21, 25, 35, and 36; but in three of these years the chief mortality occurred in a detachment at Nevis; and in the last year this little island was almost exempt, while Tortola and St. Kitts were suffering from fever. This group of islands, besides suffering frequently from epidemics, exhibits a large proportion of deaths from Yellow Fever.

As regards the influence of season, St. Lucia has alone presented any considerable excess of mortality from acute diseases, in the first six months of the year, as compared with the second, which would in part appear to depend upon its swamps being often too deeply covered with water, to be highly noxious at the latter period. In Antigua and Montserrat, and in St. Vincent's, the rate of mortality has been nearly equal in both seasons, but in these islands the mortality from fever has been very low.

In Guiana, Grenada, Barbados, Dominica, and St. Kitts, where the predominance of sickness, in the second six months, has been more marked, the mortality among 16,289 surgical cases treated in their hospitals, in the first half year, has been at the rate of 3·7 per 1,000 admissions, whereas among 13,420 similar cases, in the second half year, the rate has been 4·4 per 1,000 admissions. It thus appears that seven additional deaths took place among every ten thousand of those patients, in the season when fevers were most prevalent and fatal.

The above difference is perfectly appreciable in figures, but if it could even be demonstrated to have arisen from the exhalations of fever patients in the wards, it would not appear very formidable. These exhalations, however, would seem to have been still less prejudicial to patients suffering from chronic diseases, for in 5,764 such patients, 114 per 1,000 died in the former, while in 5,442 others, only 112 per 1,000 died in the latter period. Are we to infer then from these facts that fever may be contagious among surgical patients in the Windward Command, and not contagious among them in Jamaica? Or, that it is infectious among chronic cases in Jamaica, and not infectious in the other islands? Or that the most fatal form of fever is there neither contagious nor infectious? \* Because in an aggregate of 66,355 patients, the number appears too large to admit of the belief that one half, or any larger proportion of them, was altogether unsusceptible of Yellow Fever, or perhaps of any fever, of which the contagious, or infectious properties, and non-recurrence, have not been demonstrated for all localities. Moreover, the closer approach to equality in the number of admissions, and rate of mortality, among chronic cases, in the two periods, in the Windward command, almost proves that the marked difference in the latter respect in Jamaica was rather owing to the disparity in the admissions in the two seasons, than to any other cause, for diseases of this nature are slow in their termination.

In the Bahamas we find that the mortality among the white inhabitants of all ages, was about three times as high as in Britain, while the mortality among the white troops there, was 13 times as great as at home.

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\* The terms "infection" and "mediate contagion" are used synonymously: *matter* from the sick, in some form, and *contact* with some surface, being necessary for the communication of a disease, in either way, from person to person.

“ The principal barrack was, till lately, at Fort Charlotte, a spot notorious for its insalubrity. It is situated on the summit of the little ridge of ground in rear of the town of Nassau, and surrounded in every direction, except towards the sea, by extensive marshes, the exhalations from which, during the morning and evening, generally envelope the barrack in a dense fog,” p. 72.

“ Shortly after it was erected, in the end of the last century, nearly the *whole* of the 47th Regiment, including men, women, and children, were swept off by Yellow Fever within *a few weeks*. In 1802 the 7th Fusileers *buried* 220 out of 300 *within as short a period*, and such was the virulence of the disease, that out of 12 officers attacked, one only recovered. In the following year it again broke out, and reduced the remainder of this force to 50 men, whose lives were for a time saved by *removal to a neighbouring island*, where only *one* died in the course of *three months*; but immediately *on their return*, the commanding officer and almost *every* man of this ill-fated body fell victims to the insalubrity of the fort. For some years afterwards no European troops seem to have inhabited it; but 70 men of the 58th Regiment were sent there in 1818, who lost about 40 in six months, besides 13 out of 37 women and children; not a man of the whole force was left fit for duty; and the lives of the survivors were only saved by *removal* to a small island about a mile and a half distant. The lower rooms of this barrack proved much more fatal to the inmates than the upper, and the hospital appears to have been still more unhealthy in its site; so much so, that the white soldiers, in sickly seasons, looked upon admission into it to be equivalent to a death warrant. Though the health of the black troops is, in general, but little affected by those febrile diseases which prove so inimical to the white, even that class when occupying the same barrack” (p. 73) “suffered severely, particularly the men in the *lower rooms*. In 1828, out of 80 in these rooms, 17 died, or nearly a *fifth* of the whole, while, out of 210 in the *upper rooms*, but 8 died; and in the new barracks at Fort Nassau only 3 died out of 180,” (p. 74).

“ The barrack at Fort Nassau has always been healthy, the accommodation good, and the sickness and mortality among the troops there exceedingly low.” “ These remarks, in regard to the barracks, have been deemed necessary, in order that incorrect inferences may not be drawn as to the climate of the Bahamas, by attributing to it a degree of mortality which seems to have been, in a great measure, owing to the troops having continued, for a long series of years, to occupy a position which, from its *extreme insalubrity*, was not unaptly termed the abode of death,” (p. 74).

At Honduras the mortality of the small force of white troops stationed there, seems to hold an intermediate place between the Windward and Jamaica commands, but the Report does not furnish any particular details as to local influences on the health of the men; however, all the deaths, excepting six, occurred from remittent or Yellow Fever; intermittents, though frequent, never proving fatal. Bowel complaints are not so fatal as in other parts of the West Indies.

## CLASSIFICATION OF FEVERS, Windward Command, p. 7 of Report.

	Admitted.	Died.	Proportion of Deaths to Admissions.
Quotidian Intermittent . . . . .	24,607	149	1 in 165
Tertian „ . . . . .	1,973	11	1 in 179
Quartan „ . . . . .	133	1	1 in 133
Remittent Fevers . . . . .	17,799	1,966	1 in 9
Common continued . . . . .	16,821	726	1 in 23
Yellow Fever (Icterodes) . . . . .	774	331	1 in 2 $\frac{1}{3}$
Typhus . . . . .	48	11	1 in 4 $\frac{1}{3}$
Synochus . . . . .	8	..	0 in 8
Total . . . . .	62,163	3,195	1 in 20
Annual ratio per 1000 of Mean Strength .	717	36.9	..

Total Strength in 20 years . . . . .	86,661		
Average Annual Strength of Force . . . . .	4,333		
„ Admissions . . . . .	8,247	} from all Dis-	eases.
„ Deaths . . . . .	340		
Ratio Admitted per 1000 . . . . .	1,903		
„, Died „ . . . . .	78.5		

The foregoing table from p. 7 of the Statistical Report for the West Indies, compared with the corresponding one for Jamaica, at p. 46 of the same Report, shows such a remarkable predominance in fevers of an intermittent type in the former, that it cannot pass the notice of the most careless observer. Even when the necessary correction is made for the difference in strength of the force in the two commands, we find that five cases of intermittent occur in the Windward Station for three in Jamaica; though the rate of mortality on the admissions is almost the same in both, namely, 1 in 166 in the former, and 1 in 165 in the latter. This rate of mortality from periodic fevers is so extremely low, as to lead us to infer, that these diseases are far more benign within the tropics than in the temperate zone, but such an inference is directly opposed to general experience, in so far as regards the influence of both latitude and season on fevers of this form, which are said never to prevail epidemically beyond the isothermal line, indicating a mean annual temperature of 5° centigrade, or not rising above 10° of the same scale in summer and falling below 0° in winter, increasing in frequency and intensity, (the other conditions necessary for their prevalence being equal,) as we approach the equator.

In this dilemma we may therefore adopt, for the present, another solution of the question, by supposing that all the severe cases of periodic fever are classed under the heads of remittent and Yellow Fevers; an explanation which acquires a certain degree of probability, from the following statement of the rate of

mortality in severe epidemics of periodic fever, in other places, viz. :—

Places.	Number of Cases.	Deaths.	Authorities.
Rome . . .	96,001	8,879	Bailly.
Bordeaux . .	12,000	3,000	Contanceau.
Montiuel . .	1,352	113	Nepple.
Ercole . . .	515	115	Monfalcon.
Bona . . .	22,330	2,513	Maillot.
Total . .	132,198	14,620	= 1 in 9

If we next compare the number of cases of remittent fever among the troops in the Windward Islands, with the number in Jamaica, we find that the admissions under this head in the former, were in an equal strength, in the proportion of 18 to 65 in the latter ; and, farther, that the mortality per cent on these admissions is as 11 to 13·3.

From the nomenclature in use in the two commands, it might be inferred, that fever attended with yellowness of skin was almost unknown in Jamaica, while it was nearly 40 times more frequent in the Windward Command.

Now the occurrence of yellowness of skin in remittent, or in intermittent fevers, is a well known indication of a severe form of these discases, even in temperate climates, and is by no means a rare symptom, especially during their epidemic prevalence in the autumnal months ; hence, we may conclude, either that it has attracted more notice in the lesser islands from its comparative rarity,—an assumption, in some measure, warranted by the lower rate of mortality,—or, if it characterizes a distinct form of fever, that this peculiar disease has been nearly 40 times more frequent there than in Jamaica. On the other hand, by including the cases under the head of Yellow Fever, among the remittents in both commands,—we find that the difference in the rate of mortality from these forms of fever almost entirely vanishes, for it then amounts to 1 in 8·1 admissions in the Windward, and to 1 in 7·5 in the Jamaica Command ; and it was probably, in this way, that the reporters came to the conclusion that, remittent fevers were not more fatal, but only more prevalent, in Jamaica. *Vide Rep. p. 46.*

The far greater prevalence of continued fevers in the lesser islands, than in Jamaica, may possibly bear some intimate relation to the predominance of the quotidian type among the troops on these stations, or be, in some measure, the complement of the extraordinary difference in the number of remittent fevers, as they are, proportionally to the force employed, about five times more numerous in the one, than in the other of these localities ; though the rate of mortality on the admissions is the same in

both. On all varieties of fever taken collectively, the admissions in Jamaica, exceed those in the Windward Islands, nearly in the proportion of 9 to 7, and the deaths by little less than 3 to 1; and this difference has been so constant over a long series of years, that it could hardly be a result of the accidental importation of a distinct contagious fever, which would probably distribute its fatal gifts more equally among a class so similarly circumstanced, as to age, habits, employment, and mode of living, as British soldiers are, taken in the aggregate; especially where the difference of climate, properly so called, is scarcely to be appreciated.

The Statistical Report for Western Africa shows that the average annual rate of mortality from fever, is about four times as great in the Sierra Leone command, as in Jamaica.

The mortality from other diseases is also much higher than in that island, and here continues to form about one-sixth of the annual average, or, in other words, it has progressed in almost an equal ratio with that from fever. It may also be remarked, that the classification of fevers has been either different from that adopted in Jamaica, or that these diseases differ somewhat in the forms which they present on this coast, because intermittents form less than 13 per cent. of the admissions under the head of fever in that island; whereas they exceed 37 per cent. of such admissions at Sierra Leone, and the mortality from them is nearly twice as great as in Jamaica.

It is to an aggravated form of periodic fever, however, that the west coast of Africa owes its notorious character for insalubrity. "So generally prevalent (say the reporters, p. 9,) is remittent fever, either in its aggravated or milder forms, on this coast, that, till of late years, scarcely an European ever passed 12 months without an attack;" and after mentioning that ulcers "seemed to act as a safeguard" and a great tendency to various hæmorrhages, they add, "Except in these respects there seems to have been little difference in the disease from that which generally marks Yellow Fever of the worst type in other colonies. On this point, it may be proper, however, to remark, that, between 1824 and 1829, the black vomit is not mentioned in any of the Reports as one of the characteristics of the disease. This may perhaps induce a doubt in the minds of those who attach much importance to that symptom, as to whether the disease in these years was genuine Yellow Fever, or merely the endemial remittent of the country, but as the latter is comparatively of a mild character, the cases could scarcely have been of that type, when, in general, one-half, and, in some instances, three-fourths perished of all those attacked." "This disease did not prove equally fatal at all the stations along the coast. At the Gambia, for instance, in 1825 and 1826, three-

fourths died of all those attacked, while at Sierra Leone, during the same period, only about half the cases terminated fatally. It was also subject to remarkable fluctuations at different periods, both as regards prevalence and severity; between 1823 and 1829, for instance, it raged in the most aggravated form along the coast for several months of each year, and cut off a large proportion of the white population annually, while in 1830 and the six subsequent years it almost entirely disappeared, scarcely a case which could be denominated Yellow Fever, came under treatment." "Indeed almost every apprehension of the recurrence of fever in an aggravated form had ceased, when in 1837 and 1838, without any assignable cause, it broke out with as much virulence as during the most unhealthy epoch of the colony, and destroyed a large proportion of the white population." Though this formidable variety of fever "has in most years appeared and raged with the greatest violence during the height of the rainy season," there are several exceptions to this rule, yet it may be said to have prevailed (like ague) chiefly in the autumn and spring. There are no tabular statements to show the comparative mortality of the different seasons, at Sierra Leone, in a separate form; but at p. 13 there are ample proofs that the greatest amount of mortality and sickness occurred at the Gambia, in the last six months of each year,—and that this disease, like the Walcheren fever, did not affect those on board ship, and was therefore a product of the land,—and it also seems equally certain that it was exclusively the disease of the European, because "during the whole of this dreadful mortality, a detachment of from 40 to 50 black soldiers, of the 2nd West India Regiment, lost only one man, and had seldom any in hospital."

The evidence regarding the prevalence and mortality of fever among the white troops at the Isles de Los, is equally conclusive as to the influence of season; but the nature of the returns from other stations on the coast did not enable the reporters to separate the white from the black troops, and the total annual mortality for the Cape Coast Command, given at p. 19, is not available for the present purpose. At p. 23, however, we find a statement for the whole of this coast, which shows, that "on every 1,000 recorded," the admissions, during the first six months of the year, were to those in the second six months, as 329 to 671; and the corresponding mortality was, for these periods, as 162 to 838.

The reporters here remark, "The character of these seasons, however, is by no means uniformly manifested in this command; 1823, 1829, 1837, and 1838, were all memorable exceptions, in which fever was most prevalent and fatal during the first and second quarters, and disappeared as the season advanced, though, at these periods, the white troops were too few, for the admissions and deaths to counterbalance the preponderance in the third

and fourth quarters of other years, when that description of force was more numerous in the colony."

This inference, from the results of a calculation for the five years from 1825 to 1829, cannot be considered subversive of the statements quoted from previous parts of this Report, for, in 1829, the mortality from fever did not exceed 7 per cent. of the force, (8 in 114), nor the admissions from that disease, 77·2 per cent. during the year.

Again, if, in 1822, nine in ten died of fever, the great majority must have perished of second attacks, because, with an average strength of 10 men, there were 22 admissions. In 1823, a strength of six men presents us with eight admissions, and six deaths; but here the number appears to be too small to warrant any general conclusion, and in no other year up to 1836, inclusive, (the last which has been published) was the proportional mortality from fever,—33·4 per cent., among the smaller white force—half so great as among the largest (571 men) in 1825, when it amounted to 68 per cent. Turning from Appendix No. 1 which has furnished these figures, to the Table p. 23, we find that the presence of a greater number of soldiers, only doubled the admissions; but it multiplied the mortality five-fold, during the last six months of these years, a fact which confirms what has been already observed with regard to Jamaica,—namely, that the mortality during the latter half of the year, increases in a greater ratio than the admissions.

Neither can the inclusion of the black troops in this table, be supposed materially to vitiate the inference just drawn, because we learn from Appendix No. 2, that they lost only three men from fever during these five years, and that 30 out of 79 deaths among them, during that period, were caused by small-pox, and probably occurred chiefly in the second quarter of the year, 1827, a circumstance which would tend to diminish, rather than to increase, any error from this source.

Should the influence of the autumnal season in augmenting the number of attacks of fever on the Western Coast of Africa not appear to be sufficiently established by what has been stated above, I might adduce farther proofs from the tables at pp. 236 and 240 of Thevenot's work on "the Diseases of Europeans in hot climates, particularly in Senegal;" but the following extracts from pp. 237, 238, may possibly answer the end, viz.: "In all these localities the autumn is always the worst season; but it is in the months of October and November that the most intractable diseases appear. The second six months of the year present four-fifths of the whole annual sickness at Senegal. It is a period justly dreaded by Europeans, particularly the end of September and October." The shortness of the periods for which white troops were employed at many of the garrisons on this coast, does not enable us to determine the influence of each

locality on the number and severity of febrile diseases at the several stations, with the same degree of certainty as in the West Indies, but the comparative statement at p. 14, for the years 1825 and 1826, proves to demonstration that great differences exist even in the same years, though it does not warrant us in saying how far these differences may be of a permanent character.

The great prevalence of intermittents on the African coast, compared with the West Indies, has been previously alluded to, and the table at p. 240 of Thevenot's work, shows, that in Senegal, this type is six times more frequent than the remittent. This author also informs us, p. 244, that Yellow Fever is rare in the vicinity of Senegal, though it has twice appeared on the arid sands of Goree. It seems almost superfluous to remark, that the climate and topography of these settlements are very different from those of the English colonies to the southward, as the latter are never parched up and converted into barren wastes, for a certain portion of the year, like the plains of Senegal, —or, on the contrary, almost entirely laid under water for a month or two, at another season of every successive year, by the rivers swollen from the rains which fall in the interior; for at St. Louis the average annual fall of rain is said by Thevenot to be only 20 inches (French); while, at Sierra Leone, it seems to be more than ten times as great.

An examination of the statistical Reports of the army having shown that the prevalence and mortality of fevers in the West Indies, and on the western coast of Africa, are greatly, if not equally influenced by season and locality, the statistical Report on the Navy for the North American and West Indian station, from 1837 to 1843, was next referred to, and with the single exception of 1840, in which the admissions and deaths from fever were lower than in any other of these years, the influence of the second six months, in augmenting the number of cases, as well as the mortality of this disease, is as strikingly evinced, as in the Reports for the Army.

The quarterly returns, introduced by Dr. Bryson, in this series, show that the admissions in the first six months of these years, were 2,600; in the second six months, 3,731; and the deaths in the former period, were 92; in the latter, 230; the mortality, therefore, was in the ratio of 3.53 per cent. to the admissions in the one, and 6.16 per cent. in the other,—a result in perfect accordance with the observations already made, that the mortality increases in a greater ratio than the attacks, at this season.

It could scarcely be expected that any striking illustration of the influence of locality, in developing or aggravating fevers, would be afforded by the returns for the navy, in consequence of the frequent changes of place incident to this service; but the

Table, No. 3, at p. 129, exhibits the effects of this climate in no favourable light, when contrasted with the Table at p. 41 for the South American station.

The Returns in the Appendix also show, that fever is both more frequent and fatal in steam vessels than in others; but it would be necessary to know the strength of the crews, in every instance, before the amount of this apparent difference could be determined. Thevenot gives at p. 177 some remarkable proofs of the mortality from fever among the crews of vessels in the rivers of Western Africa; and the experience of the royal navy, has only too frequently exemplified the danger of exploring them.

It would be quite superfluous to enter into any details for the purpose of showing, that in the United States, and in Spain, Yellow Fever epidemics have been, in all instances, strictly limited to the last six months of the year,—that being the period in which the endemic fever of the West Indies, and of Western Africa, is also most prevalent and fatal; but a cursory glance at the table given by Arejula, for the calamitous year 1804 may not be altogether useless.

From that document we learn that the epidemic broke out at Malaga on the 29th of June;—at Velez, and at Antequera,—the former about 10, the latter about 18 miles distant from that city, it appeared on the second of August; at Alicant on the 9th; at Montilla on the 11th; and at Arcos on the 14th of August.

A reference to a map of Spain will show, that if Velez and Antequera are within a few hours walk of Malaga; Montilla and Arcos are remote, and not even on the main line of communication. Assuming Montilla to be a new point of departure, it travelled north 20 miles to Cordova, between the 11th of August and the 28th September; for Arejula ridicules the idea of its being introduced by “un lino,” from Malaga, and says the communication was as free as between Madrid and Carabanchel, or, to use an illustration near home,—between London and Hampton.

Its progress, however, in this direction, had been comparatively rapid, for it took 10 days longer to reach Ecija, only 10 miles west of Montilla. On the 28th of August, the inhabitants of Cadiz were variously blaming Malaga, Gibraltar, or Havannah, for sending them this fatal scourge, while it had been quietly established for a fortnight at Arcos, on the Guadalete, about 18 miles from them, and half that distance from Xeres, which it nevertheless did not reach until the 23rd of September.

From Alicant it travelled 12 miles to Guardamar, at the mouth of the Segura, between the middle of August and the 21st of October; and finally it managed to get over the 10 miles of ground from Paterna to Ximena between the 30th of August and 27th of October. If the cross roads of Spain be not very good at the present day, they were probably worse in 1804; we need not, therefore, be greatly surprised that Yellow Fever turned

aside from many places which appeared to lay almost on its path ; but having reached Cordova, with one of the best roads in the country *apparently* open, the epidemic could not ascend the central table land of Spain, (one of the highest in Europe,) to visit the capital ; probably from the same obstacle that has foiled it for centuries, in gaining the table land of Mexico from its home at Vera Cruz.

It would thus appear that the outbreaks of Yellow Fever are about as little subject to any rule of gradual progression, as those of Cholera were in its late visit to this country ; and it may now be well to inquire if its ravages were not as capricious as its course. With regard to this point, the same table shows that Velez Malaga lost about 3 in 7 of its inhabitants, while Cordova lost 1 in 130 ; Vera lost half its population, whereas Grenada lost 1 in 180 ; at Carthagena de Levante 1 in 3 perished ; at Ximena 1 in 150 ; at Malaga, nearly 1 in 3 inhabitants died ; at Ecija, less than 1 in 10.

Thus in the same epidemic, in the same country, whether we compare large towns with small villages, or large towns with one another, the contrast in the mortality is equally great as in Cholera, and the influence of locality can scarcely be doubted.

It was almost necessary to have some such evidence as this from the Peninsula, because the prevalence of Yellow Fever there in 1804, has been at least generally admitted ; and the inflammatory, bilio-inflammatory, or complicated remittent, has not hitherto been accused of usurping the place of the genuine Bulam, in that year in Spain. Arejula very quaintly informs us (p. 154), that none of the "profesores" of medicine and surgery, knew the disease to be an intruder when they first saw it in Cadiz in 1800 ; this also occurred at Medina Sidonia in 1801 ; and again at Malaga in 1803, "*notwithstanding such patients had fallen into the hands of able physicians, who had treated a considerable number of cases of this same fever in 1800.*"

Indeed, he so frequently mentions remissions, and at p. 168, many of the signs of pernicious fever, as occurring in the course of this disease, that his own belief in its distinct or specific character, cannot be deemed fixed, especially when we consider the treatment (by Quina) he so strongly recommended ; and at p. 147, he literally refers its differential diagnosis to the attention of the medical officers of the navy, in his indecision as to its identity with the non-contagious "vomito prieto." Had the learned doctor been alive, he might possibly have found some satisfactory information on this point, in the valuable Reports of Dr. Bryson on the navy, and of his fellow labourers of the army, in the field of statistical inquiry ; here, however, the question of diagnosis may rest for the present ; but the skilled in nice distinctions, or, as Arejula calls them, "pure scholastic differences," will find enough to gratify their curiosity in this way, in the articles on Remittent Fever, and Hæmagastric Pestilence, by

Dr. Copland; and at p. 509, of the Monography of Mongellaz, they may read a case of pernicious intermitten, detailed by Trousseau, in which the black matter vomited, was "tout à fait identique," with the black vomit of patients suffering from Yellow Fever.

It is a commonly received opinion that previous attacks of Yellow Fever, or simply length of residence in those particular localities where that disease frequently appears, independently of any attack, will afford to persons so circumstanced, a certain degree of immunity during its epidemic prevalence. This belief, however, has received many rude shocks of late years, from the statements and figures of the statistical reports, and the authority of writers, who like M. Catel, (p. 80,) think "the typhus icterodes may attack the same individual several times, provided he be in that state of health which favours the development of this inflammation." I have been tempted, therefore, to examine the question a little, in order to ascertain how far this opinion may be founded on fact. The difficulty (or utter impossibility) of distinguishing the slighter forms of Yellow Fever, from other febrile diseases of the localities where it appears, unless by its epidemic prevalence, is generally admitted; as well as the rarity of recovery, after attacks attended with ("los accidentes," Arej., p. 159) yellowness of skin and black vomit. Dr. Blair informs us (p. 83) that these symptoms coincide in only 6.95 per cent. of the number attacked; and the great mortality, 84.72 per cent., among the so characterized cases, proves the inutility of relying on these rare examples—about 1 per cent. of the total cases—for the appreciation of this immunity. The only sources of positive information then, really available for this purpose, are the returns in the statistical Reports, especially for Jamaica; and as no one has hitherto attempted to prove that length of residence there, diminishes the frequency or mortality of any other disease of that climate, it may be assumed that any advantage to be secured by acclimatization, extends only to febrile diseases.

In estimating the influence of length of residence on the mortality among the troops serving in Jamaica, we surely cannot be warranted in excluding the years in which epidemic fever prevailed, because many writers suppose the alleged immunity to be exclusively limited to the periods of such visitations.

Neither can we fairly consider a soldier, or stranger, to be fully inured to a tropical climate, by a residence of less duration than from three to five years. If the latter period be adopted as affording a greater probability that a regiment had passed through, at least one epidemic season; on turning to Table LXXVI, p. 92, of the Statistical Report, we find that the average mortality, during the first five years of service, was in the ratio of 147.7 per 1000, whereas in the second five years, it was

104·7 per 1000 of the strength. If we now examine Table LXXVII., p. 93, from which the great epidemic years are excluded, we find that the mortality, in the first five years of service, was 102 per 1000, and in the second five years, 82 per 1000 of the strength. This result, which is in accordance with common belief, presents us with a curious fact,—namely, that the immunity, or advantage, enjoyed by the acclimated, is not confined to epidemic years, though in such years it appears to be about twice as great as in ordinary years; the diminution of the mortality, amounting in the former to 4, and in the latter to 2 per cent. of the force respectively. As the tables include a period of 10 years only, these results might perhaps be modified, to some extent, by observations continued over a long series of years, but the numbers under observation, and the acknowledged accuracy of the returns, may be admitted to give this alleged immunity a degree of authenticity, which it could never have acquired from individual experience, limited as that experience necessarily must have been, to the few survivors of *duly characterised* attacks of Yellow Fever.

I may further remark, that the remittent fever of Jamaica, which has been generally considered to attack the same person again and again, either, does not in reality do so, or, if old residents suffer from repeated attacks, it is certain that such subsequent attacks must be much less fatal; and the difference between the epidemic and endemic forms of fever, would thus appear to be simply in the amount of protection afforded by each variety of the disease, as regards the rate of mortality,—this property being common to both. It would further appear from the observations of M. Boudin (*Ess. de Géographie Med.* p. 46) that pernicious fevers, the periodic nature of which cannot be disputed, have never, in his experience, returned in the same individual a second time.

I have examined the Returns for Jamaica, only, with reference to this immunity, because the great differences in the rate of mortality from fever, in the several islands in the Windward Command, must prevent the same importance being attached to the latter Returns, with regard to this question, so long as the troops shall be frequently moved from one island or government to another; moreover, diseases of the stomach and bowels are four times more fatal in the Windward Command, (in Dominica 13 times) than in Jamaica, and it is generally allowed that these diseases are aggravated by length of residence.

The comparative exemption of the negro race from the fatal fevers of Sierra Leone is demonstrated by the Table at p. 16 of the Report; but the returns for the West Indies prove, that in general, the African is there more liable to fever than in his native land. Nor will it be easily credited by those who have not looked into these Reports that his rate of mortality from that disease is twice, thrice, or even five times as great, in some

of these islands as in others ; and although the maximum of this rate does not coincide in every island with the maximum among the white troops, there is nevertheless such an approximation between the maximum and the minimum, for both races, on the same stations (p. 51) as would lead us to infer that they both suffer from the same local causes. A striking fact of this kind at Nassau, already quoted from the Report, seems of itself so conclusive, as to render abortive any attempt to found on these occasional occurrences a specific distinction\* between Yellow and Remittent Fevers, an application of them (to diagnosis) by the way, which might lead to the inference that Yellow Fever was more than four times as prevalent at St Kitts, &c., as at Sierra Leone.

Medical men entertain very different opinions regarding the cause of Yellow Fever ; some attribute the disease exclusively to a specific contagion either mediate or immediate ; some believe that it may become contagious at one time, or place, from accidental circumstances, and not be contagious in another ; and many maintain that the cause is of local origin, and that the disease is never contagious.

This question of origin has given rise to much bitter controversy among medical men, but I shall not enter into any detail upon a subject which has occupied the attention of others better qualified for the task.

However, it appears necessary to remark, that the great mass of evidence contained in the statistical Reports of the Army, is adverse to the doctrine of contagion ; while the evidence in the Reports for the Navy is, in a few instances, of an opposite character ; a result which might naturally be expected from the circumstances under which the disease had there been observed.

The Reports agree in showing that remissions have been present in a great majority of the epidemics of fever in the West Indies and Western Africa ; that these epidemics may not occur for years in some of the worst localities ; that they may return without any assignable cause, and prevail with various degrees of intensity, for long periods ; and that black vomit may not be noticed, or in a few cases only, in the most fatal epidemics on the African Coast.

These differences have been alleged, by the contagionists, to depend upon the prevalence of two distinct diseases, either simultaneously or at different times, in the same locality ; and Dr. Copland says, " Much of the misconception which formerly existed respecting these maladies, was owing to the unfortunate names given to them, especially to the term Yellow Fever, which, as will be seen from what has been adduced from various

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\* Copland, Dictionary of Practical Medicine, p. 951, vol. i., and note, p. 173, vol. iii.

writers, was generally applied to this pestilence, although yellowness of the surface was more remarkable in the severe endemic remittent not only of Africa and America, but also of Spain and the shores of the Mediterranean.” \*

The yellowness of surface, which is here allowed to be more remarkable in the severe remittent, forms only an instalment of what was justly due to that disease, for the same author is obliged to concede a share of the black vomit, and the best observers have established its claim to a great number of cases of an apparently continued form, among the fevers of hot climates,—the pseudo-continued of some French authors. It would thus appear that remittent, had at least as good grounds as Yellow Fever, to complain of an unfortunate name, which has deprived it of its undoubted rights. The reporters, therefore, were justified in adhering to the nomenclature in general use, because a little more or a little less black vomit occurring in these fevers, is a very insufficient distinction at the bedside of a patient, and no unequivocal diagnostic symptoms present themselves during the course of the disease. The short duration of Yellow Fever has been relied on as a distinctive characteristic of that disease, but in an epidemic, evidently from local causes, at La Graverie (Calvados) in 1809, death took place, in a majority of the cases, in 6, 8, 12, 15, 20, and 24 hours from the attack, and I am not aware that Yellow Fever has ever proved so rapidly fatal; its duration, however, differs in different epidemics.† Moreover the (disputed) infectious quality is admitted to be so easily dissipated by winds, that it may not be manifest; there may be no sufficient reduction of temperature, to test the power of endurance of the disease in this way; and in order to be enlightened by the immunity from second attack, we must wait for the next epidemic. Indeed the more this question is examined, the greater does the difficulty of establishing any invariable characteristics of Yellow Fever appear to be; but it is unnecessary to prosecute this subject farther, as the distinct nature of Yellow Fever is founded upon belief, rather than observation.

It has been already stated that periodic fevers are rare in high northern latitudes, and that they increase in frequency and severity as we advance southwards; the other conditions necessary for their prevalence being equal. It is not in frequency and severity alone, however, that these diseases become modified, by the influence of a high temperature in augmenting the intensity of the cause. Their forms also are so much changed, that the periodic character would be no longer recognized by those who had observed them only in northern climates. In this progress towards the equator the tertian is supplanted by the quotidian, the latter by the remittent, and this again in its turn becomes

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\* Dr. Copland, Dictionary of Practical Medicine, vol. iii., p. 166.

† Montfalcon, p. 478.

partly superseded by the subintransient, the pernicious, and the continued, either with or without yellowness of skin and black vomit; the mortality also keeping pace with these transformations. These conversions into different types occur not only in passing from high to low latitudes, but they appear in temperate climates, according to the season of the year, and the violence of the various epidemics.

Nor are these all the circumstances which compel periodic fevers to undergo such evolutions, for even within the tropics we may sometimes see the same series, inverted in its order, in ascending from the low lands on the coast, to considerable altitudes. In conforming therefore to this influence of season in temperate climates, and in ceasing to appear at certain altitudes within the tropics, as well as by its symptoms, Yellow Fever betrays an alliance with periodic fevers. The affinity, however, does not stop here, for it has been observed that both varieties of the disease attack strangers, especially those from northern countries, in a violent or fatal form, while the native or long resident population, may be suffering from a milder grade of the disease, a fact pointed out long ago by many authors, more particularly by Arejula, and lately confirmed by the numerical statements of Dr. Blair.

Another feature of resemblance likewise throws its weight into the same scale, namely, the influence of the cause, whatever that may be, upon the health of the lower animals as well as of man. Arejula, in his description of Yellow Fever, frequently refers to such occurrences, particularly at p. 236, and Sir W. Pym, Drs. Gillkrest and Smith, have noticed this fact in the Gibraltar epidemic of 1828; and Dr. King more recently at Boa Vista. On the contrary, M.M. Dupuy, of Alfort, Rodet, and Hurtrel d'Arboval, have observed intermittent in the horse and other animals. In speaking of the notorious insalubrity of the district round Brouage, M. Melier says, "The mortality not only strikes down man, it also reaches animals. In this melancholy country, as in all others which are insalubrious, epizootic diseases advance in line with epidemics."

The opinion that the so-called Yellow Fever is merely a variety of the pernicious forms\* of periodic fever, appears to be gaining ground, and it derives no small support from the sudden occurrence of adventitious symptoms ("los accidentes," Arejula, pp. 160 and 168) during the progress of the disease; from the

\* The term pernicious has been applied exclusively to fevers, fundamentally of a periodic character, whether their type be intermittent, remittent, or apparently continued, in which violent and extremely dangerous symptoms, such as furious delirium, coma, the state called algid, or vomiting and purging of watery or bloody fluids, &c., &c., come on suddenly during the progress of the disease, and either prove rapidly fatal, or if the patient should recover, the transition from extreme danger to safety, or health, is as abrupt as the invasion was unexpected. Moreover, fevers of this irregular type, and remittents, are far more rarely followed by such sequelæ, as dropsies, &c., than the ordinary intermittent, especially when the latter assumes a chronic form.

nearly similar rate of mortality; and above all, from the appearance of undeniable cases\* of pernicious fever during Yellow Fever epidemics. A case of pernicious intermittent has already been referred to, in which, says M. Trousseau, the black vomit was "perfectly identical" with that which is thrown up by patients labouring under Yellow Fever, and here, at least, the competency of the observer cannot be disputed. Pugnet mentions vomiting of black matter as a fatal symptom in the Demel-Mouia, (a form of pernicious fever) in Egypt; Lancisi also records the appearance of brown and black vomit, and stools, in an epidemic at Pesaro, in 1708; and the latest observations of this occurrence in Europe appear to be those of M. Garnier, in the French hospitals, at Rome, and even at Versailles, as I have been informed by M. Boudin.† The sudden appearance of black vomit, after what is commonly called the fatal lull (remission?) in Yellow Fever, is so similar to the rapid invasion of pernicious symptoms, in epidemics arising from paludal infection, that it must strike the most inattentive observer. These remarkable transitions, moreover, are exclusively peculiar to the pernicious variety of periodic fevers, and the risk of a return in this pernicious form appears to be at least as inconsiderable as in Yellow Fever.

After tracing some general features of resemblance between diseases said to be different in their nature, and distinct in their causes, it may be well to cast a glance on this latter part of the subject, premising that I consider the cause to be the same for both varieties, namely, paludal infection, or malaria, for its sources are not always obviously paludal, at least, in external aspect.

The morbid agent, which gives rise to the numerous varieties of fever of an essentially periodic character, is generally believed to be the product of those reactions, which take place between the remains of organic matter and inorganic substances, when they are brought in contact under the conditions necessary for such reactions. A few have denied the efficacy of this agent, or even its existence; M. Boudin, and one or two others have maintained that the effluvia of certain living plants will cause fever,—and Dr. Wilson has advanced the opinion, that the slow changes going on in ligneous fibre will produce the same effect; and a remarkable occurrence on board the "Vestal" appears to support his views. This ship had taken on board a quantity of green firewood, which was supposed to cause a disagreeable smell, perceived soon after in the hold. The wood was, in consequence, got on deck, and some men, chiefly marines, were employed to strip off the bark, all of whom were taken ill "in the course of two, or at most three days" afterwards.‡ Other accidents of a somewhat similar nature might be adduced in

\* Pugnet, Arejula, Lewis, Ruz, Dutrolau, Bertulus, Gillkrest, Brown, and Jackson.

† Bulletin de l'Acad. de Med.—Aout 1850.

‡ Return on Health of the Navy from 1837 to 1843, p. 107.

favour of this theory, which, in reality, differs but little from that generally adopted, and appears to rest on as good grounds as the opinion of some contagionists, who assert that the specific poison which causes Yellow Fever was originally generated on board crowded slavers; or that the disease may be produced by the effluvia of putrid cod-fish.

The Yellow Fever has so frequently appeared in sea-ports, as to give a certain air of probability to the assertions of those, who believe that this disease must be always imported.\* Contagionists, however, as well as their opponents, have almost entirely overlooked the fact, that such ports, generally situated near the embouchures of rivers, or on narrow inlets having rivers running into them, are more exposed than other places, to the causes of insalubrity, which depend on the mixture of fresh with salt water.

Now, this mixture of fresh with salt water is one of the conditions which is most favourable for the production of malaria,—probably in part from the destruction of organic life which it occasions, and in part from the elements which it furnishes for chemical reactions. The following illustration, taken from the report of M. Melier,† will decide this point.

The locality of the experiment, for such it may be truly called, was the Maremma of Lucca on the shore of the Mediterranean. The ground is divided into three principal basins, interspersed with numerous lakes and pools, which for ages had been inundated from time to time by the sea, thus causing the mixture of fresh with salt water. The insalubrity of this district, we are told, was so great, that “inevitable death” was the consequence of passing a single night during the months of August and September, in this pestilential locality. In order to remedy this sad state of affairs, in 1740, a valved sluice was placed in the Burlamanca,—the channel by which the sea-water entered the principal basin,—so constructed as to shut out the tides when they rose, and to allow the fresh water to escape when they ebbed. These works were finished in 1741. The success was so complete, that fevers which had never before failed to appear, ceased in the following year, and from that time the district became healthy. The village of Viareggio, previously abandoned, became an important place, and a favourite summer residence of the first families of Lucca. In 1768 and 1769, the fevers suddenly reappeared, as in their worst periods. “What had happened? Only this: the sluice had got out of repair, and the waters were again mixed.” The sluice was repaired, and fever disappeared. The deaths which had risen to 1 in 15 were reduced in the following year to 1 in 40 inhabitants. The same accident occurred in 1784-5, with similar consequences, and the evil was remedied in the same manner.

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\* See note on the “Eclair,” at the end of this paper.

† Rapport sur les Marais Salants, p. 77.

Similar sluices were established at Cinquala in 1812, at Motrona in 1819, and at Tonfalo in 1821. Everywhere the success was the same; the experiment is as conclusive as it is undeniable. Similar results have since been experienced at other places in Italy, and in France.

Here, then, we have a demonstrated source of insalubrity, especially affecting towns situated on the low alluvial banks of tidal rivers, and also calculated to throw much light on the occurrence of fever on board ships, without the intervention of an imported contagion. In towns differently situated, whether on the coast, or in the interior, however dissimilar outward appearances may be, the sources of insalubrity, where they exist, would appear to be fundamentally the same. In the latter localities, the constituents of the earth's surface, which are known to vary from one point to another, supply the necessary elements, either in a state of solution, in the waters arising from different sources, or in saline substances soluble in the water, which falls in the form of rain, and the consequent chemical reactions between the organic and inorganic materials, take place under the same laws as elsewhere. It would be as vain to pretend that the insalubrity of any considerable number of localities had been traced by actual experiment to this cause, as it would be preposterous to affirm that the organic and inorganic constituents of the earth's surface, the composition of the waters, the influence of temperature, of light, of electric currents, and the direction and force of the winds,—all essential elements in such investigations,—had been determined with even an approximation to precision in any one situation. The fact, however, has been ascertained for a few districts in Italy, and its applicability to others may fairly be inferred.

All countries of volcanic formation, for example, contain in the rocks and soil an unusually large proportion of soluble saline substances; their crust is generally more permeable to water than that of other regions of different origin; they abound in mineral springs, and in many of them the vegetation is particularly luxuriant. In these countries, in places which present no external signs of marshes, or evident sources of insalubrity, fevers of a periodic character are known to occur, in an aggravated form, and sometimes to decimate the inhabitants. Many places in Italy, the whole of the West India Islands, and no small portion of the shores of the Caribbean Sea; several islands on the West Coast of Africa, and portions of its shore, as well as localities elsewhere; have presented the world with a melancholy series of proofs in favour of this doctrine. "It is an ascertained fact," says M. Melier, "that all stagnant waters are not equally dangerous; if there are some, which it is enough to pass in order to be infected, there are also others which are harmless, so to speak, or only give rise occasionally to a few

slight fevers. On what does this difference depend?" He informs us, that he has undertaken an inquiry into this subject, which has not as yet been carried so far as to enable him to say "even with reserve, that the difference depends on this circumstance,—that in the one case the waters contain sulphates in large proportion, while in the others they are present only in trifling quantities, or not at all." "But," he adds, "we are inclined to believe that it is so." The quantity of water present also materially modifies the action of the solar heat, in effecting these changes, which give rise to the morbid agent, and M. Carrière shows us that surfaces which are but thinly covered, or simply moist, are known to be the most dangerous situations. "There nothing modifies or arrests the action of caloric, and the elaboration which is its result, is always coincident with an increase in intensity of the morbid influence."

The various changes in the forms of organic life, so constantly present in stagnant waters, may also play an important part in the production of malaria. In salt-marshes, for example; when the water attains a certain degree of concentration, it acquires a rose colour, and may even become as red as blood from the development of a new infusorial animalcule, which appears after the aboriginal races have perished. At this period the water exhales the peculiar odour of the violet or iris, which is occasionally so strong as to cause headach and other feelings of uneasiness in some of the labourers.\* In fresh waters, likewise, similar changes, among the infusorial and vegetable forms they contain, would appear to take place under certain conditions of temperature and evaporation; and their relative degrees of insalubrity may perhaps depend on such transformations, as well as on the partial exposure of their alluvial beds to the direct action of solar heat.

The pond of Lindre-Basse, in the department of the Meurthe, affords a curious illustration of the effects of the different conditions, under which malaria is generated, in modifying diseases arising from paludal infection. This fish-pond managed according to the triennial rotation system, common in Sologne, is two years under water, and one year dry. In the first year it is half filled, and gives rise to intermittent fevers; in the second year it is full, and typhoid fevers prevail; in the third year, after being fished, it is left dry and cultivated as a field, and in this year carbuncular affections appear. These diseases have succeeded one another as regularly and invariably as the different states of the pond, for a period of 16 years;† and the idea naturally suggests itself, that diseases which have a common

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\* Melier sur les Marais Salants, pp. 20 and 49.

† A change in the rotation in 1848-9 has altered the order of succession of these diseases.—*Comptes Rendus*, 1850.

origin, must have more or less a common nature, however much they may differ in outward appearance.

The material morbid agent resulting from the reactions between the remains of organized structures and inorganic matter, is considered to be sulphuretted hydrogen, which the experiments of Chevreul, Henry, Fontan, Daniell, and others, have proved to be one of the products obtained from these decompositions, when sulphates are present.

"At this limit," says M. Carrière, "direct investigation stops. It may, perhaps, be able (yet) to discover if this substance, the existence of which is proved, does not undergo new modifications during its contact with the air, before that final one, when deposited in the tissues, and mixed with the vital fluids, it eventually becomes the agent of this intoxication."

However this may be, experimental physiology has shown that one cubic inch of this gas in 1500 cubic inches of air will kill a bird, and one cubic inch in 800 will in no long time kill a dog.\* Matteucci likewise informs us that "sulphuretted hydrogen is the only body, which having acted on the blood, even in very small quantities, renders this fluid incapable of being arterialized by oxygen.†"

Moreover, MM. Dujardin and Didiot have observed, that after intense fevers the aeration of the blood during the separation of the fibrine, by beating that fluid, does not restore its bright red colour as in the physiological state.‡

Malaria is capable of being conveyed by the winds from the place where it is generated to others more or less remote. This fact, while it affords a proof of the material existence of the agent, also explains the insalubrity of localities where the sources of this poison may not exist. M. Melier gives a remarkable example of this at Marennes. "When the wind blows from the east, north-east, or north, that is, in such a direction as to carry away from the town the effluvia of the waste marshes, which are all to the westward of it, fevers are rare. On the contrary, when it blows from the west, south-west, or south, namely, in the direction that, passing over the marshes; it carries the effluvia upon Marennes, fevers are sure to appear." "At St. Agnant, situated on the opposite side of the marshes to Marennes, things are completely reversed, it is the east wind that there brings fevers with it, the same wind which keeps them away from Marennes." The distance to which the effluvia may be conveyed appears to be considerable, for, at the season when they are most abundant, Rochefort, four or five miles from the marshes, suffers from the south-west wind, which carries the effluvia upon it from Brouage. This carrying power of the winds appears to be in

\* Carpenter, Hum. Physiology, p. 601.

† Lect. on Phys. Phenom. of living beings, p. 127.

‡ Haspel, *Maladies de l'Algerie*. p. 376.

some degree dependent on their hygrometric state; and the accumulation of the electric fluid near the surface of the earth, during humid states of the atmosphere,\* may perhaps favour the development of the effluvia.

The whole subject, however, requires both extensive and accurate observation; and this alone can enable us to give a satisfactory reply to a question often asked, namely,—with marshes always present, why should one year be more unhealthy than another? At present we can only reply thus,—the morbid influence of marshes, and other sources of malaria, is so manifestly subordinate to season, that we must infer there has been some difference in the meteorological phenomena, our acquaintance with this part of the subject, being altogether insufficient to enable us to affirm, that any two years have been in all respects alike. Nay, a distinguished authority in such matters informs us that “since the beginning of the world, the temperature and humidity of the atmosphere have perhaps not been twice in identically the same circumstances for eight consecutive days.”† What value, then, can we attach to conclusions drawn from the observation of states so variable, that even the mean annual temperature has scarcely been determined, with all the necessary precision, at a few points on the surface of our globe, while the other meteorological phenomena are yet more imperfectly known? Besides, the few diseases, which are generally acknowledged to be propagated by a specific contagion, frequently make little progress, without the aid of those peculiar states of the atmosphere, which, in our ignorance of their nature, have been called epidemic constitutions; and if these conditions are admitted to be necessary in the one case, why should they be considered uninfluential in the other?

If medical topography has so frequently been unsuccessful in reconciling the notorious insalubrity of some localities with their external appearances, its failures have arisen from the imperfection of the means of investigation employed,—not from the impossibility of arriving at more positive results. The facts adduced in the works of M.M. Melier and Carrière, show that chemistry has hitherto been too often either neglected, or misapplied in such researches.

The recent labours of Professor C. F. Schönbein, however, seem to throw an important light on this subject. The Professor, after showing “that the quantity of ozone present in the atmosphere at the same place, varies at different times,” proceeds to prove, by experiment, that this agent decomposes the fœtid products of the putrefactive processes going on in animal bodies after life has ceased; which products, if not destroyed, could hardly fail, in the process of time, to accumulate in such quantities as

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\* Arago and Schumler, Carrière, p. 76.

† Quetelet, *Letters on the Theory of Probabilities*, &c., p. 106.

to render the air "poisonous, and unfit for sustaining animal life." He further adds, that "above all other seasons winter is most distinguished by the abundance of atmospheric ozone;" and also, "that the higher strata of the atmosphere are more ozoniferous than the lower ones." Now, general experience attests that those diseases, which are almost universally considered to arise from malaria, are comparatively rare in winter, and that they also diminish in frequency (other circumstances being equal) with the increased altitude of the locality.

M. Schönbein, therefore, justly remarks, "As the generation of some diseases, such as Yellow Fever, &c., seems to be connected with certain seasons and geographical positions, it would, I think, be worth while to ascertain, by comparative ozonometric observations, whether [such] diseases bear any relation to the ozoniferous state of that portion of the atmosphere within which they happen to occur. Considering the great obscurity in which the causes of most diseases are as yet enveloped, and as it is, nevertheless, highly probable that some at least, if not many of them, are the effects of chemical agents which exist in the atmosphere, and have a great physiological effect, *i. e.*, act in most minute quantities with great energy upon the animal constitution, scientific physicians and physiologists should earnestly follow out any train of research which promises to increase our insight into the connexion between abnormal physiological phenomena, and physical or chemical agents." \*

Those, therefore, who may possess the amount of scientific knowledge necessary in such inquiries, have the field open; but they must always recollect, that it is not by ingenious speculation, but by exact and long-continued observation of the physical phenomena presented on the spot, that any precise or durable information on a subject of so much importance can ever be acquired.

Before quitting the subject, it may be well to appreciate, if possible, the value of the evidence adduced, as well by the advocates of contagion in Yellow Fever, as by those who oppose that doctrine. The contagionists hold the proof on their side of the question, though founded on an assumption, to be perfectly conclusive, and they further affirm that a few positive facts must be entirely subversive of any amount of negative evidence.

This estimate of the value of these presumed facts, so improperly called positive, is altogether erroneous, for in order to attain certainty, in contagion, as in everything else, the cause must be shown to be necessary, and the effect to be invariable. Then, and only then, can the relation between cause and effect be positive or certain. The only sources of human knowledge absolutely certain, are the simple self-evident truths which

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\* *Medico Chirurg. Trans.*, vol. xxxiv., p. 219.

instantly carry conviction to the mind, such as axioms, and mathematical demonstration. All beyond these narrow limits are mere questions of probability. The absolute exemption of very many persons exposed to the risk (hypothetical or otherwise) of contagion in Yellow Fever, rests on the direct evidence of our senses, and possesses, in consequence, a degree of conclusiveness which cannot be set aside by sophistry. It has therefore such a superiority over all conclusions, *a posteriori*, as no supposed instances of communication of the disease can ever pretend to, so long as numerous exceptions to that alleged rule shall present themselves. Nor would the accumulation of 10,000 indisputable examples of the communication of Yellow Fever, from person to person, make its transmission in any given case a matter of certainty, though this event would have become more or less probable from the amount of facts in its favour. If rigorous observation, and ample experience, under conditions free from all exception, should ever raise the testimony in favour of the contagious nature of Yellow Fever, to such a position as this, in the scale of evidence, it may appear less ridiculous to talk of conclusions, *a posteriori*, as positive facts. Because the chances of the disease being transmitted from one person to another, may then be made the subject of calculation as a question of probability, which, like all similar questions, must be liable to fluctuation within *fixed limits of possible error*. The controversy as hitherto carried on, has been a war of words and conflicting opinions, in which, all the facts coming under direct observation are to be found on the side of non-contagion. Hitherto, the bold assertion of the contagionist has been received without examination by the unthinking, and with avidity by the timid, who have been thereby erroneously encouraged to look for that safety in isolation, which can only be secured by hastening from the unhealthy locality, and which the troops in Jamaica have so largely experienced in their new quarters,—the mountainous districts of the island.

I have the honour to be,

My Lords and Gentlemen,

Your obedient servant,

A. BROWNE, M.D.,

Surgeon Half-Pay, 37th Regiment.

To the General Board of Health,

&c.

&c.

*Note on the alleged Importation of Yellow Fever into Boa Vista, by the Steam Ship "Eclair," 1845.*

The appearance of Yellow Fever at Boa Vista, after the arrival of the "*Eclair*" in the harbour of Porto Sal Rey on the 21st of August 1845, has been often referred to as a proof

that the disease was introduced into the island by that ship. The evidence, however, as stated by Dr. McWilliam, now Medical Officer to the Customs Department in London, seems by no means conclusive on this point.

Immediately on the steamer anchoring, John Jamieson, now also of the Customs in London, was sent off to her by the Consul, but did not go on board,—as Captain Estcourt had doubts whether the ship would be admitted to pratique, on account of the sickness prevailing on board at the time.

Jamieson reported to the Consul what had passed. Mr. Rendall then waited on the Governor, and it was ordered that Mr. Kenny, a medical practitioner, should visit the ship. Mr. Kenny, accompanied by Jamieson, went off accordingly, and both went on board. Mr. Kenny, having gone below to visit the sick, returned ashore with Jamieson, and they were soon followed by Captain Estcourt and the chief engineer. "Captain Estcourt went first to the Consul's, and afterwards to Mr. Macaulay's." "The engineer, after only a short stay on shore, went off to the ship;" where *he* had been is not stated. "The Kroomen remained about the pier until the captain returned to his ship in the evening," p. 13. In reply to the next question, Jamieson says, "I went on board the same night with the engineer, and remained there till half-past 10, when I returned to the shore." Jamieson brought ashore in his boat "at least a dozen bags of soiled linen belonging to the officers of the ship," which he deposited in the Consul's store-rooms for the night, and gave to the washerwomen of the town next morning. The list of washerwomen given to Dr. McWilliam by Jamieson contains 17 names; that in Dr. King's Report, p. 16, contains 19 names. Four of the washerwomen died; and Dr. McWilliam says, at p. 82, "I have examined the whole of the survivors, and have found that 2 were attacked late in October, 5 in November, 2 in December, 3 in January, and 1 not until some time in February. None of the deaths took place until fever was general in Porto Sal Rey; so that in none of these cases can the occurrence of fever be fairly attributed to infectious matter conveyed by the linen." We must agree with Dr. McWilliam in this opinion, all the more that Anna Santa, in reply to Query 75, "Have you had fever?" answers, "No;" and Delphina Barbara, in reply to Query 112, "Have you had fever during or after the 'Eclair' was here?" also answers, "No." Dr. McWilliam, who examined the washerwomen some time between the 1st and 30th of April, seems, nevertheless, to have found 13 of them who had had fever, and *one* who had not.

Can the number of washerwomen be incorrect,—or the summary of their evidence given at p. 82 of Dr. McWilliam's Report?

Having disposed of the washerwomen, we find that Captain Estcourt went to live at Mr. Macaulay's on the 25th of August,

and he nobly devoted the whole of his time to his sick crew, until he was taken ill himself, on the 11th of September, and at once conveyed to the fort, pp. 78, 79. Here, then, was another channel through which a contagious disease might have been introduced, but as no bad results followed, it is not even alluded to. Mr. Kenny, who visited the sick on board once, and in the fort "more than once,—indeed frequently" (p. 81); as well as Dr. Almeida, who saw them once at the latter place, are not supposed to have introduced the contagion any more than Jamieson, the engineer, or Captain Estcourt. The sick and crew of the "Eclair" were landed at the fort on the 31st of August, and the officers (gun-room, midshipmen, and warrant) occupied a house in Porto Sal Rey at the same time. The house is close to the sea, in the part of the town called Santa Barbara, and "the officers and their servants were daily in various parts of the town." In this house, "some of the other officers, the captain's cook, and some other servants, were taken ill;" but they, as well as the purser at Mr. Kenny's, "were sent to the fort as soon as they were attacked," and no bad consequences arose from this, the best of all the sources for a contagious disease to spread from. Previous to this, however, leave had been given "to the petty officers and a few of the sailors" (p. 82) to go ashore. The seamen seemed to have resorted chiefly to the house of a publican called Georgio, in Porto Sal Rey. "It is remarkable that this man was attacked with head-ache and general fever on the evening of the day he was visited by the 'Eclair's' people;" but it is not so remarkable that, among the many friends who visited him during his indisposition, Anna Gaspar and Rosinha San Antao should have had slight fever shortly after being at the gin-shop; for Anna Gaspar says (367) she had seen no sick, nor does Antao say *she* had. The only thing marvellous in the case of Georgio, is how the "Eclair's" people, who were all well themselves, could have given him head-ache and a hot skin, unless by treating him with too much grog, a failing common with sailors. This channel of introduction is presumed to be suspicious; but "the pear was not yet ripe."

Let us now see how things went on at the fort with the soldiers on guard there.

Corporal Perez, of the first guard, though indisposed while in the fort with the "Eclair's" people, was put on duty after his return to Sal Rey; but, getting worse, was relieved, and was ill about a month: his wife, however, who attended him, did not fall ill until the sickness became general. He says he was not visited by many during his illness, and though Dr. Almeida saw him once, his disease could not have excited any suspicion in the mind of the doctor: and Dr. McWilliam never put a question to him as to the nature of the disease, which evidently occurred early in September; and no attempt has been made to trace the

epidemic to this source, as it did not appear until the middle of October. Pedro Manoel, of the same guard, was not sick at the fort, nor at Sal Rey, until there was a good deal of fever there. Antonio dos Santos, also of this guard, was not sick at the fort, nor until three weeks after he had left it. This man's fever must have occurred in September; and this channel of introduction is likewise abandoned. The reason for all this seems pretty obvious; for Dr. McWilliam (p. 83) tells us Dr. Almeida saw Perez "during his convalescence:" and the doctor says, shortly after his only visit to the fort, he went to Fundas das Figueiras, and returned to Sal Rey on the 19th or 20th of October. As Perez left the fort on the 3rd or 4th of September, it is not probable that he was again put on duty before the 6th or 7th of the month, consequently his disease must have been very trivial if he was convalescent before Dr. Almeida left the place; and it seems probable that the doctor only saw him in the last ten days of October. Santo's case is equally unsatisfactory, as the disease did not break out in barracks until long after, and without dates such histories are always inconclusive.

The second guard was six or seven days in the fort while the "Eclair's" sick were there; but the Corporal, Silva, was not sick in the fort, and was not sure when he got sick, but there were several sick before he was. Manoel Antonio Alves, of this second guard, was not ill in the fort, and assisted to bury the two Portuguese soldiers about the 21st or 22nd of September. On the latter day he was sent back to the fort on duty, where he remained a day and a half only, having been attacked and taken to barracks at the end of that time (*i.e.* about the 24th), where he was ill four or five days, and weak much longer. Luis Briza, the third soldier of this guard, died of fever on the 12th of November, but it is not said how long he had been sick.

The third guard at the fort consisted of two Portuguese soldiers, and Miguel Barbosa; the two former died there, one on the 20th, and one on the 21st of September. Barbosa, the only survivor, "is not quite sure, but thinks he was about six or seven days" in the fort with the "Eclair's" sick, and for several days after. He says (218 and 219) that Corporal Agosthino and Roque were taken ill the day after the "Eclair" left,\* and (220) that the corporal died on the third, and Roque on the fourth, day of their fever, which would show that the corporal died on the 17th and the private Roque on the 18th of September: Now, Major Mascarenhas shows that the one died on the 20th, and the other on the 21st of the month; and Pedro Manoel says

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\* "But the soldiers had only a week before they were seized with fever come from Porto Sal Rey, where, at least in a theoretic view, the condition of the soil during and after the rainy season is such as may cause fever. Assuredly many bad fevers have been attributed to less obvious causes."—*Report*, p. 105. Should we suppose that these men had carried "the germs of fever in them" from Porto Sal Rey to the Fort, the incubative stage would only be about half as long as in the cases of Barbosa and Manoel from the Fort.

he attended them "upwards of three days;" having been sent to the fort for that purpose. This same Manoel says, he and Barbosa remained about four days on guard at the fort after the death of their comrades, therefore they must have been relieved about the 25th of September. Barbosa and Manoel, on being relieved, were sent to a house in the Pao de Varella, where they stayed about eight days. It was during this stay in the Pao de Varella, "with the germs of fever in them," that Barbosa and Manoel are supposed to have communicated Yellow Fever to Anna Gallinha. Barbosa, however, was not confined to bed until a day after he went to barracks, and was then only three or four days ill; and the dates, in this history, prove that he must have gone to barracks about the 3rd or 4th of October, and was laid up about the 4th or 5th of the month: but, in reply to Query 238*a*, he says Luis Briza was sick in barracks at the same time with himself. Luis Briza, therefore, was ill with Yellow Fever about 36 days; for we know that Briza died on the 12th of November: the evidence of Barbosa, if it be worth anything, would prove that Yellow Fever is not so "rapid in its course" as some great authorities imagine it to be. His comrade Manoel, after carrying the germs of fever in him, for four days at the fort, and eight days at Pao de Varella, was, nevertheless, three or four days in barracks before he was laid up, consequently he was attacked on the 7th or 8th of October. From these details it appears that Corporal Perez carried fever to the barracks about the 4th of September; Privates Santos and Alves about the 24th or 25th of that month; and Barbosa and Manoel in the first week of October: but Yellow Fever *would not attack* the soldiers before it *suit*ed its own convenience to do so; it was, therefore, useless to look for the origin of the disease in this quarter.

The fourth guard must have gone to the fort about the 25th of September, and as it remained 12 or 13 days there, it must have returned about the 8th of October; consequently, Pedro Gonzalves, of this guard, must have had the germs of fever in him for six weeks, if they were derived from this source, as he is said not to have been "taken ill until the first of our (the guard) returning to Porto Sal Rey," and he died on the 24th of November.

The first guard was 4 days in the fort with the sick of the "Eclair;" the second guard, 6 or 7 days, and this guard left it all well; Barbosa, the only survivor of the third guard, thinks, but is not quite sure, that he was there with the "Eclair's" people "about 6 or 7 days." If Barbosa be right, all the rest must be wrong, for the "Eclair's" crew were only 14 days in the fort; but his evidence regarding Luis Briza must make the advocates of "rapid course" doubt the value of his testimony.

Dr. McWilliam states, p. 86, that "the disease had attacked several persons in Porto Sal Rey proper, by the end of October, and some of the soldiers were sick in barracks at this time."

This statement seems to be fully warranted by the evidence, especially by that of Dr. Almeida, who says (1565) upon returning to Porto Sal Rey on the 19th or 20th of October—only three or four days after the death of Anna Gallinha—"I found about 20 people sick." All intercourse with the "Eclair's" men had ceased between seven and eight weeks before a single death took place in barracks; and no fever of a severe or fatal nature had appeared there earlier than in the town, notwithstanding, soldiers and they alone, with the exception of Mr. Kenny, who died on the 27th of November, had been in frequent or close contact with the sick belonging to this unfortunate ship. No doubt some soldiers had been unwell at an earlier period, but their maladies had been mild in comparison, a circumstance not unusual in epidemics from malaria; though not much in accordance with the progress of diseases possessing "highly infectious qualities" (p. 110). Indeed, these qualities must have been of extraordinary virulence when their "germs" could infect Anna Gallinha, who had only been in contact with the sick while the disease was in the stage of incubation.

Manoel Affonso, the subject of the next fatal attack in Porto Sal Rey, is *supposed* to have had an unlucky blanket from the "Eclair" in his possession for a month, and to have visited Anna Gallinha, before he was taken ill, in this same Pao de Varella, the only locality in Porto Sal Rey that Yellow Fever would, at this time, enter; but it shortly became less scrupulous in its visits.

The first case of fever among the 9 labourers, from Porto Sal Rey, employed on board the "Eclair," occurred in December.

The first case among the 2 from Estacia occurred when fever was general there. Two cases among the 30 men from Rabil, employed on board the "Eclair," occurred in September, one in October, and the others later. Luis Pathi, of Rabil, was the first attacked of all the labourers who had been on board the "Eclair." This man who was *supposed* to have taken a *blanket*, or *something* from the "Eclair," had, according to his evidence, been employed on board eight days. He also says he was taken ill about three days after the "Eclair" left; not at Rabil, however, but at Moradinha on the opposite side of the ravine, where he was attending a festival. The date of his attack therefore was the 16th or 17th of September, and the first eight days of his illness were passed in a friend's house at Moradinha; but the germs of this "highly infectious" fever would do no mischief to his friends in this village just yet. At the end of eight days he was conveyed to Rabil, consequently about the 25th of September. Dr. McWilliam takes the date of attack from his reply to 654, but from that to 656 it appears less positive; and as the festival was held on the 14th, it was probably on that day, as stated by Dr. King.

Pathi says he was ill in his own house "nearly three weeks,"

and that his daughter was taken ill about the beginning of October, and died in three days with black vomit. Dr. McWilliam, however, says, p. 89, that she was taken ill ten or eleven days after Pathi's return, or about the 5th or 6th of October; therefore she must have died on the 8th or 9th. The next girl was taken ill four days after the death of the first, therefore on the 12th or 13th, and she died in four days; about the same time with Anna Gallinha, or earlier if Pathi was taken ill on the 14th of September.

Thus two deaths from black vomit fever had occurred in the "most crowded part" of the most unhealthy village in Boa Vista, at least as early as any similar case in Porto Sal Rey.

The reply of José Marques to 1327, shows that a child of Manoel Fachina had died before either of Pathi's children; but no questions were put to Fachina on this point, perhaps because he had been employed "only at the coal heap,"—and had never been on board the "*Eclair*,"—or seen the sick at the fort. If in the case of Pathi's children the father himself, or the *blanket*, or *something*, may be suspected to have communicated the disease; at Fachina's there was no *blanket*, or *nothing*, to explain the nature of the occurrence.

This is not the only difficulty in the way, for there is nothing, worthy the name of proof, to show that Pathi's fever was connected with the sick of the "*Eclair*." Pathi was certainly employed on board, and was there when the sick were embarked, but he had not, like Antonio Angela (725), Joaquim Pathi (744), or Antonio Maria Simoa (594), assisted in getting the sick on board, nor does it appear that he was near them; and it requires an implicit belief in the "highly infectious qualities" of the disease, to suppose he was taken suddenly ill with the same fever, which was so ineffectual at Moradinha, where Dr. McWilliam says, p. 91, "no other case of fever occurred until some time in December." In a note at the foot of p. 90 however, he says, "there was fever there three weeks after Pathi left the village," or as the dates will have it, about the 16th of October. These statements do not agree, but on either hypothesis the disease would do no mischief until brought to the village some time in December, by Magdalena Spence, and even then it would not attack one-half of the inhabitants (548), cutting off only two residents in a population of 101. Moreover we are told that fever comes there occasionally in the end of the year.

At Estação de Baixo fever prevailed, but from some ill-explained circumstance, it was so mild as to cut off only three persons in a population of 256, and one of the three had been in Porto Sal Rey. Those who have little faith in vaccination, would deem the discovery of a locality where small-pox would be equally tractable, a matter of no small importance.

Dr. Almeida says, in reply to 1566, that he had seen "in all,

about 150 sick," yet he did not suffer from fever. However he seems to have become prudent, for he left Porto Sal Rey for Boa Esperança, and from this place he went to Fundo das Figueras on the 19th of November. In answer to 1574, he says there were no sick at this place on his arrival, but in a few days after, a man called "da Cruz," "came from Rabil, and was taken sick in the course of a few days; in a week or ten days after this his mother and sister were taken sick, and both died." Dr. McWilliam, p. 92, says this man had "slept in a bed at Rabil in which two persons had died." A "few days" from the 19th of November, and "a few days," and "a week or ten days" added together, would necessarily bring us to the first week in December; but Dr. McWilliam, p. 93, says, "To whatever cause it was due, there was no farther extension of the fever until the middle of December, when a man, called Antonio José, came to the place from Joao Gallego, where he had left his sister on her death-bed from fever." Dr. Almeida satisfactorily explains all this by the precautionary measures he adopted (1574), and adds, "by these means I kept the fever under until the middle of December;" and in reply to 1575, extends the time to "at least three weeks;" and in the next answer goes on to tell how José rendered all his precautions nugatory. An intelligent witness, Lieut. Santos, of the artillery battalion, replies to 496, that he went to the northern villages on the 29th of November, and to 497 he answers, "at Cabeça dos Tharafes I found many sick; at Fundo das Figueras only *one* sick; and I am not aware that there were at this time any sick in Joao Gallego." On his return to the villages about the end of December Joao Gallego contained several sick, Cabeça dos Tharafes contained a great many sick, and the people of Fundo das Figueras had established a cordon sanitaire between Cabeça dos Tharafes and their own village. To 501, "Then was Fundo das Figueras quite healthy at this time?" he says, "No, there were several sick people in a house that had been converted into a hospital." Dr. Almeida left the village on the 23rd of December, and (1581) says "there were a good many cases" at that time. It would appear from all this that Joao Gallego had *no* sick on the 29th of November, (in despite of 2 soldiers having been sick there on the 3rd of the month (476); while Fundo das Figueras had *one*; yet the former had not only become sickly,—but also infected Fundo das Figueiras within 24 days,—notwithstanding the cordon and other precautionary measures, which, imperfect as they were, had yet, according to Dr. Almeida, kept the disease under "at least three weeks."

It has been seen that Dr. McWilliam says, Antonio José got into the village about the middle of December: though well on his arrival "he was soon laid up," and had done all this mischief within a week; though "da Cruz" had been comparatively

harmless in November. Such is this history of the double importation of fever into Fundo das Figueiras, and of the efficacy of cordons and separation of the sick, &c.

At Porto Sal Rey itself, with the exception of the "remarkable" case of Georgio in August, doubts might be raised as to who was the first sufferer from fever, for Romess says (315) I was taken ill about the same time as Anna Gallinha; and Jamieson confirms this statement. But in reply to (318) Romess says he was taken ill "three days, it might be a day or two more," after his wife got the clothes of Manoel and Barbosa to wash; and she got these clothes (317) the day the two soldiers came to the Pao de Varella,—consequently about the 24th or 25th of September,—and the intervals stated would carry his attack back to the 1st of October. The reply to 331 likewise shows, that Gallinha, Affonso, and Bent, were all ill at the time Romess was; and this might very well be the case, as (319) he tells us he was ill "nearly two months." The replies to 683 and 1253 would seem to render it by no means a "*positive fact*" that even Luis Pathi was the very first person attacked in Rabil; but without dates no evidence on questions of priority as to time, can be considered worthy of the smallest consideration.

Not a few discrepancies are seen in the evidence given by various witnesses, and in many places we find blunders of a grosser kind; for example, Dr. Almeida (1564) denies that he was in the fort a second time, and assigns his absence from Porto Sal Rey as a reason for this; whereas, Barbosa (228) and Manoel (237) assert that Dr. Almeida saw the two Portuguese soldiers there during their fatal sickness.

Dr. McWilliam, in his Remarks on the report of Dr. King, relies on the same assumption, viz., that Yellow Fever was introduced into Porto Sal Rey by the soldiers, Miguel Barbosa and Pedro Manoel, and by them communicated during their temporary sojourn in the Pao de Varella, to Anna Gallinha, Michael Affonso, and others. In a note at p. 1 of the Remarks, the date of the return of those soldiers from the fort, is fixed by Mr. Rendall as the 24th of September; their stay in the Pao de Varella is given by Dr. McWilliam as eight days, consequently to the 2nd of October; and Manoel had been three or four days in barracks before he was taken ill; the date of his attack therefore was the 6th of October. This man was lodging in the house of a publican, during his illness of 15 days, and as he had *black vomit* according to his own account, this remarkable occurrence, from the publicity of such an abode, must have been well known before Anna Gallinha was taken ill, because recovery within a week after such a symptom would be perfectly miraculous. This, then, was one of the first cases, but no one seems to have noticed this remarkable circumstance, not even Jamieson, or the pub-

lican Joana. In short, the whole story is improbable, and wants corroborative testimony. Barbosa passed through his fever in barracks, and was taken ill and went to bed on the 3rd of October, but fever did not prevail in barracks until some four weeks after, and the first death there, according to Major Mascarenhas, occurred on the 6th of November. Lieut. Santos in reply to 491, p. 34, corroborates the Major, by stating that nearly all the soldiers were ill on the 24th of November. Barbosa likewise says, Luis Briza was ill at the same time he, Barbosa, was; now Briza died on the 12th of November; consequently he was ill 40 days before his death, otherwise Barbosa cannot be credited. Besides, mortal attacks of Yellow Fever *rarely* last so long.

I have before pointed out earlier cases of sickness in barracks, mentioned in the Report of Dr. McWilliam, but no bad results having followed at that time, these cases appear to have escaped further observation.

The soldier Alves, in reply to 192, names two men who assisted him to bury his comrades who died in the fort; yet no notice is taken of those men, who were some time in the boat with, and must have even handled, the corpses.

Dr. McWilliam says, p. 10, "For it is quite certain that the fever did not show itself until (and it is to be marked not until) the soldiers came to Beira." Admitted,—but how came it to pass that the cases of Perez, and all those already noticed as occurring in the house occupied by the officers in Porto Sal Rey, of Capt. Estcourt, &c., to be so innocuous up to this time?

Dr. King says, Barbosa and Manoel were in Beira 17 days, now if this were the case, Anna Gallinha must have been taken ill the day they left, and at least one day before Barbosa; but this would not admit of Barbosa's fever standing in the relation of cause, to Gallinha's fever as effect; or in the widest and wildest order of causation, the attacks must have been simultaneous; and this would be an absurdity in most people's view of such an event as the transmission of fever from one person to another. And what an incubative stage, 10 or 18 days!

The statement at p. 42, 43, of Dr. McWilliam's Report, given by Luis Pathi himself, affords no ground whatever for the assumption in the last four lines, p. 11, of the Remarks, that *he* assisted in hoisting in the sick, nor is this important piece of information to be found in the Report;\* but I have mentioned Joaquim Pathi and two others who were so employed. This however, gives an air of probability to the further statement, in the first lines of p. 12 of the Remarks, as to Pathi's case "being a positive instance of infection, which is not to be controverted by any number of mere negative results." The inconclusiveness of such reason-

\* See Report. pp. 108 and 110, where this case is noticed as an exceptional one of infection by fomites.

ing is demonstrated in Poisson's work "*Probabilité des Jugemens*," and the formulæ are given to determine the probability of conclusions *à posteriori*.

In p. 12 of Dr. McWilliam's Remarks, he repeats that Joao Chico Fernandez introduced the fever into Estacia, but we find Gregorio Zavars in reply to 606, p. 39, of Report, disputing this claim with Chico Fernandez, and the reply to 610 would seem to throw doubt on Fernandez' being even the second case.

Dr. McWilliam has recourse to the hypothesis of infection for the reason assigned in p. 8 of Remarks, that he "failed to discover the least evidence of the cause in the earth, in the air, or in anything animate or inanimate on that island, or on the small island, unconnected with the 'Eclair,' &c.:" and the second conclusion, p. 111 of Report, states, "That although there exists in the Island of Boa Vista a physical cause capable of producing remittent fever, yet it does not appear that that cause was in action when fever broke out in September 1845, and that the island was quite healthy when the 'Eclair' arrived there."\* At the top of p. 9 of Remarks, however, he says, "By the time Anna Gallinha was taken ill,† much rain had fallen; the weather had become more hot, and, in short, there now (but not before this) existed the recognised elements for malarious evolution;" and he might have added with equal truth, "now, but not before this," did the fever extend in Porto Sal Rey, or indeed anywhere in Boa Vista. At p. 6 of the Remarks, he quotes the evidence of Capt. Buckle, of the "Growler," to show "a little rain fell once or twice during the night while we were there." But does Dr. McWilliam's knowledge of the conditions necessary for the production of malaria, enable him to decide that this "little" was not enough?

Dr. McWilliam's conclusions, given at p. 15 of Remarks, cannot be said to rest on any probable evidence, still less on "positive" facts, for he expressly admits, as we have seen, the existence of endemial sources of fever, and at the time of Anna Gallinha's attack of "the recognised elements for malarious evolution." The second conclusion is quite beside the question at issue, as malarious fevers are always local, though sometimes spreading wider than at others.

The evidence in support of the third conclusion, instead of amounting to proof, is most unsatisfactory; and a large majority of those who will carefully examine the testimony must, I presume, feel a respect for the judicious reserve of Sir W. Burnett, as to the deductions to be drawn therefrom, which few perhaps would willingly accord to the late qualified opinion of the Royal College of Physicians.

\* On the 21st of August.

† On the 12th of October.

The statement of Mr. Rendall, the Consul, at p. 6 of Report, proves that clean bills of health were first refused to vessels leaving Boa Vista on the 20th of November,—the same day the disease was declared to be contagious; nevertheless, it does not appear that the want of quarantine had been injuriously felt in any of the neighbouring islands, though restrictions on intercourse had been so beneficial for a time at Fundo das Figueiras, as to keep the disease under, though actually inside the cordon. The Governor-General and his suite of 60 Europeans remained in the island up to the 1st of December, yet they all escaped so far as has been made known, nor were they able to carry the disease along with them, pp. 8 and 110. The epidemy among the cattle arose from *some* cause, but, as it could scarcely be attributed to a specific contagion communicated by man, it has not been alluded to in the Report, or Remarks.

The differences in the ratio of mortality (p. 94) among the different classes of residents were apparently contrary to all previous experience in contagious diseases; for the most opulent and best lodged suffered most in proportion; because they were strangers. The mildness of the disease in some villages compared with its severity in others, would also appear capricious in a disease propagated by contagion, and therefore to a great extent beyond the influence of locality. Moreover, the evidence has been obtained chiefly from the most ignorant classes of the population.

Notwithstanding the conclusion to the contrary in this Report, the opinion of the noble-minded Don José Miguel de Noronha, that the disease “was perfectly endemic,” has at least as good evidence in its favour as anything adduced on the opposite side of the question; and if his generous philanthropy has not been rewarded, for so fearlessly aiding the noble efforts of Captain Estcourt to alleviate the sufferings and avert the destruction of his crew, it has at least merited some testimony of England’s gratitude.

(Signed)      A. B.

## APPENDIX No. IV.

NOTICE OF THE PROCEEDINGS OF A BOARD  
OF INQUIRY HELD AT THE OFFICE OF THE  
ARMY MEDICAL DEPARTMENT, 1849-50, ON THE  
NATURE OF YELLOW FEVER.

A SERIES of questions relative to Yellow Fever having been issued from the Privy Council Office, and forwarded through the Horse Guards to the Director-General of the Army Medical Department, with directions that a Board of Inquiry should be constituted to deliver their opinions on the subject, and such Board having been formed accordingly, the General Board of Health at the termination of the inquiry, requested to be furnished with a copy of the proceedings and evidence taken on this occasion. In reply to this request, a copy of the opinions delivered by the several Members of the Board was forwarded from the Horse Guards, with an intimation that the evidence might be taken by any officer of the General Board who should attend for that purpose at the office of the Army Medical Department.

Some of the documents being very voluminous, the General Board instructed one of their clerks on whom they could rely, to make a summary of the proceedings and evidence which is here given as follows :—

It appears that the Board consisted of the following Members :—

*Dr. Andrew Smith*, Deputy Inspector-General of Hospitals (President), employed as professional Assistant to the Director-General.\*

*Dr. Thomas Spence*, Staff Surgeon, 1st Class, on hospital duty at Chatham.†

*John Millar*, Esq., Staff Surgeon, 1st Class, previous to the assembling of the Board, on the recruiting service at Coventry ; after the termination of the proceedings, at Glasgow ; and since deceased.

*Dr. William H. Burrell*, Staff Surgeon, 1st Class, on the recruiting service in London.‡

*Henry Pilleau*, Esq., Staff Surgeon 2nd Class (who acted as Secretary to the Board), employed at the office of the department under the President.

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\* Now Inspector-General, and Superintendent of the Army Medical Department.

† Now professional assistant to the Superintendent of the Department.

‡ Now stationed at Malta.

The first proceeding of the Board was to “attentively read and examine all official or other documents available, and likely to elucidate the subject to be investigated.”

No formal minutes of the proceedings were made, nor was any available record kept of the titles of the documents read, but each member appears to have taken such notes as he might deem expedient.

The second proceeding was “to receive oral evidence of Army Medical Officers, and other persons competent to furnish information on the points in question;” but 18 gentlemen only were examined, all of whom are Army Medical Officers, viz.:—

Name.	Rank.	Date of Examination.
Dr. McLean . . .	Deputy Inspector . . . . .	Oct. 11, 1849.
Dr. Dods . . .	Surgeon 36th Regiment . . . . .	Oct. 18 „
Mr. Parry . . .	Surgeon, H. P., 4th Foot . . . . .	Oct. 31 „
Dr. Kelly . . .	Surgeon, Ordnance Medical Dept. . . . .	Nov. 8 „
Dr. Daniell . . .	Staff Assistant Surgeon . . . . .	Jan. 9 1850.
Dr. Hawkey . . .	Staff Surgeon, 1st Class . . . . .	Jan. 10 „
Mr. Bradford . . .	Surgeon 1st Battalion 23rd Fusileers . . . . .	Jan. 11 „
Mr. Connell . . .	Staff Surgeon, 2nd Class . . . . .	Jan. 14 „
Dr. Lloyd . . .	Surgeon 36th Regiment, H. P. . . . .	Jan. 15 & 19, 1850.
Mr. Brown . . .	Surgeon Grenadier Guards . . . . .	Jan. 16 „
Dr. Jameson . . .	Staff Surgeon, 2nd Class . . . . .	Jan. 17 „
Dr. Arthur . . .	Deputy Inspector-General, H. P. . . . .	Jan. 21 „
Dr. Millingen . . .	Surgeon 31st Regiment . . . . .	Jan. 22 „
Dr. Webb . . .	Staff Assistant Surgeon . . . . .	Jan. 23 „
Mr. Blakeney . . .	Staff Surgeon, 2nd Class . . . . .	Jan. 28 „
Sir W. Pym . . .	Inspector General, H. P., &c. . . . .	Jan. 29 „
Dr. Gillkrest . . .	Inspector General, H. P. . . . .	Feb. 5 <i>et seq.</i> 1850.
Mr. Hugh Fraser . . .	Surgeon, H. P., 60th Rifles . . . . .	Feb. 12 „

After an interval of two months, the opinions of the Board were given, preceded by each question separately, as follows—

“As regards the first question,—‘*Is Yellow or Bulam Fever a distinct disease, or only an aggravated form of the marsh or remittent fever of warm climates?*’

“The President and three members, viz., Dr. Spence, Mr. Millar, and Mr. Pilleau, are of opinion that the Yellow or Bulam Fever is a distinct disease, and not an aggravated form of the marsh or remittent fever of warm climates.

“The fourth member, Dr. Burrell, is of opinion that the Yellow or Bulam Fever is an aggravated or occasional form of the ordinary fevers, continued, remittent, and sometimes intermittent; is identical with these in nature, and causes, and is not more peculiar or less indigenous to the latitudes in which it commonly occurs than the common continued fever is to this country.

“As regards the second question, namely—‘*Does one attack of Yellow or Bulam Fever give, like small-pox, immunity from a second attack, except in very rare instances?*’

"The President and one of the members, Mr. Pilleau, consider that one attack of Yellow or Bulam Fever does give immunity from a second attack, except in rare instances.

"A second member, Dr. Spence, is of opinion that an attack of Yellow or Bulam Fever protects the constitution very greatly from liability to a second, but the exact extent requires further observation.

"A third member, Mr. Millar, considers that one attack of Yellow or Bulam Fever does give, like small-pox, immunity from a second attack, except in very rare instances.

"And the other member, Dr. Burrell, is of opinion that an attack of Yellow or Bulam Fever, like length of residence, enables the constitution to resist to a considerable extent a recurrence of fever in the same form, but gives little or no protection against what he considers other forms of the same disease, or those which attack the acclimated.

"As regards the third question—*'Is Yellow or Bulam Fever a contagious disease?'*

"The President and two members, Dr. Spence and Mr. Pilleau, are of opinion that Yellow or Bulam Fever has frequently manifested a contagious power; but that on many occasions the contagious power, if in existence, could not be detected by the most careful observation.

"One member, Mr. Millar, is of opinion that Yellow or Bulam Fever is a contagious disease.

"And the other member, Dr. Burrell, considers that nothing sufficient has been adduced before this Board to determine affirmatively a question of so much importance to humanity and science, and believes the Yellow or Bulam Fever to be absolutely and universally non-contagious.

"As regards the fourth point, namely—*'Is Yellow or Bulam Fever capable of being imported?'*

"The President and three members, Dr. Spence, Mr. Millar, and Mr. Pilleau, are of opinion that Yellow or Bulam Fever is capable of being imported.

"The other member, Dr. Burrell, is of opinion that Yellow or Bulam Fever is not capable of being imported."

It may be remarked with regard to the evidence on which the first of these opinions was founded, that

Dr. McLean, although he does not give any answer directly to the point, says, among other observations of a similar tendency, that he "thinks the same causes that will produce remittent in an old resident, will produce Yellow Fever in new residents."

Mr. Parry "considers the disease in question [Yellow Fever] was an aggravated form of the common remittent fever."

Dr. Daniell's opinion does not appear very clearly, but he says, "in the remittent fever the skin was generally yellowish."

Dr. Hawkey says, "I consider Yellow Fever, remittent fever, and intermittent fever, only grades of one and the same disease."

Mr. Bradford says, "fever prevailed during these periods in the forms of intermittent and remittent; and the aggravated form of the latter, viz., Yellow Fever."

Mr. Connell "had an opportunity of seeing intermittent, remittent, and malignant remittent or Yellow Fever, at several stations."

Dr. Lloyd observes, "I call the aggravated form of remittent that which exhibits black vomit."

Dr. Jameson observes, "I consider the three latter [intermittent, remittent, and Yellow Fever] the same fever, only differing in degree."

Dr. Gillkrest says, "I believe the epidemic Yellow Fever of Gibraltar, in 1828, was of the same family as the severe remittent fever of tropical climates."

Mr. Hugh Fraser considers "remittent fever and Yellow Fever to belong to the same family of diseases."

The testimony of two other witnesses, viz., Dr. Dods and Mr. Brown, would appear to favour a similar opinion; whilst the remaining six only are decided in considering Remittent and Yellow Fever to be distinct diseases.

With regard to the opinions of the Board on the second question, it appears that all parties were agreed as to the fact that Yellow Fever, like several other fevers, gives a considerable degree of immunity from a second attack, though not an absolute immunity,

With reference to the third and fourth questions, Dr. McLean remarks on the case of the "Crocodile," that "almost all employed in the hold and coal-hold were generally attacked with the fever. Thinks the Army Medical Officers at Barbados considered it was not contagious. Barrack-room where the majority of the cases occurred was immediately in the neighbourhood of a drain."

Dr. Dods, after stating that whilst stationed at Demarara, a sailor from Barbados arrived with Yellow Fever, and died in the Seamen's Hospital (the disease at the time prevailing in the latter island), says, "no case originated in hospital after his admission."

Mr. Parry "ascribes the cause of this disease [fever with black vomit] to malaria;" and states, that "he never saw any fever there [the West Indies] which he considers contagious."

Dr. Kelly "saw no case [of Yellow Fever] which he could have considered to have been produced by any virus emitted from a patient labouring under the disease. Does not think there is any infectious fever in Jamaica."

Dr. Daniell states, "no other person in the house I lived, as far as I know, suffered from the fever under which I laboured [Yellow Fever] at Sierra Leone, either before or after the attack."

Dr. Hawkey says, "I consider the cause of remittent fever to be some exhalation from the earth, at a time when a peculiar state of the atmosphere prevails. I believe Yellow Fever depends upon the same cause."

Mr. Bradford says, "I consider the disease [Yellow Fever] was caused by a local vitiation of the air; the severe cases arose in different parts of the barracks at the same time. I never believed, from my own observation, that the disease was propagated from one person to another."

Mr. Connell offers no direct opinion, but his testimony may be considered as calculated to support non-contagionist views.

Dr. Lloyd says, "I never knew of any case during my residence in the West Indies (15 years) where the Yellow Fever was propagated from one person to another. I do not believe that fevers arising from palludal or terrestrial sources can become contagious under any circumstances."

Mr. Brown says, "I could not attribute a single case [of Yellow Fever] that I saw to contagion."

Dr. Jameson says, "when black vomit prevailed as an epidemic fever at different times, I never remember a single case of a hospital orderly, or a patient in hospital with any other disease, attacked with black vomit fever, except, I think, one or two. I have never seen black vomit fever occur above 4,000 feet."

Dr. Webb says, "I do not think the disease [Yellow Fever] is capable of propagating itself by contagion."

Mr. Blakeney states, "the patients with Yellow Fever were not isolated from the other patients. I do not recollect any case of Yellow Fever originating in the hospital. We never thought about contagion. Nor did either men or officers think of contagion, so far as I know. From my present impressions I should not separate Yellow Fever cases from the rest of the patients."

Dr. Gillkrest says, "all my experience, &c., and the result of my deep reflection on the subject, as well as the consideration of its history in different parts of the world, go fully to the strongest conviction upon my mind of Yellow Fever not being contagious under any circumstances whatever."

Mr. Fraser says, "I never saw anything in the epidemic of Gibraltar, in 1828, that led me to believe the disease was contagious, but the contrary."

Dr. Millingen's opinion may probably be considered doubtful, for he observes, "At that time [*i. e.*, in the midst of a Yellow Fever epidemic], I did not consider that the disease was contagious, though now, after reflection, I should not consider myself doing my duty if I did not take precautions which at that time I did not believe necessary."

Dr. Arthur, on the other hand, states that "the ordinary or marsh remittent fever occurs in all situations where marshes or uncleared grounds exist;" that he *never* found *black vomit*, either

before or after death, in cases of *remittent* fever; and yet afterwards (speaking of a severe visitation of "*remittent* fever," when "from 60 to 100 cases were in hospital at the same time)," he proceeds, "I recollect some cases on that occasion *had black vomit*, and one man in particular was taken very ill in hospital, and on examination *I found on the mattress on which he was lying marks of the black vomit, which was black vomit from a former patient who had died shortly before*. I could not account for the aggravation of disease of this patient, who was apparently getting well till this time."

The ORAL TESTIMONY delivered before the Board was as follows:—

*Evidence of Dr. M'Lean, Deputy-Inspector.—October 11, 1849.*

Has seen Yellow Fever in Barbados in 1843; in Demerara in 1843 and 1844. Saw a good many cases at Barbados, which occurred among the crew of the troop-ship "*Crocodile*." She arrived with a detachment of the 81st from one of the other West India Islands—St. Kitts. Troops had landed a month before Dr. M'Lean's arrival, and were under canvass near the old Naval Hospital. Does not think that any of the soldiers who landed from that vessel had Yellow Fever in camp. At the time of Dr. M'Lean's arrival (1st February 1843) the ship's crew, from having suffered on board from Yellow Fever, had been landed and encamped on the beach, about one-fourth of a mile from the garrison. Cases were from time to time admitted from the camp into the detachment hospital, which were considered genuine cases of Yellow Fever. Black vomit in almost every fatal case preceded death. Crew about 120 strong. Thinks about 10 cases out of the 120 were admitted from the camp. Cases continued to be admitted till about the end of March. Men were not allowed to enter the garrison or town. During the time the crew were under canvass, some of the sailors were from time to time employed on board of the ship, after the ship had been fumigated and otherwise cleansed. Almost all employed in the hold and coal-hold were generally attacked with the fever, and were sent on shore to the hospital. Sailors from merchants' service volunteered their services to enter the naval service, and worked on board the "*Crocodile*" during the day, and passed their nights elsewhere. Knows that two of these men were attacked with the fever. Were sent on shore to the detachment hospital. The disease had ceased to spread in the ship when Dr. M'Lean left the island on the 22nd or 23rd April. The ship had undergone a second purification, and the coal hold had been cleansed out. Some of the officers and men remained on board during the whole period the ship was in the bay. Thinks that some of the men, and knows that some of the officers, were attacked subsequent to the camp

being formed on shore. Some believe the disease existed in the vessel before the troops bound for Barbados embarked at St. Kitts; others considered it originated after the troops embarked. Understood the disease was prevailing at St. Kitts when the troops left that island. The surgeon of the ship was of opinion that the disease was contagious, and brought on board ship. Thinks the army medical officers at Barbados considered it was not contagious. No Yellow Fever at the time prevailed in the garrison or among the civilians. While the men were ill with the fever in the hospital, one of the 46th Regiment was attacked in the same building, but in a lower story. The surgeon of the regiment considered it Yellow Fever. Dr. M'Lean and Dr. Birrell did not. About 30 soldiers were at the time in the hospital. The soldiers were cautioned not to mix with the sick sailors; but no steps were adopted to prevent intercourse. Convalescent sailors were placed in a ward with convalescent soldiers. Dr. Hardy attended the sick sailors, and continued healthy. None of the orderlies in attendance upon the sick sailors or soldiers, excepting the soldier already mentioned, took the disease. Thinks that the fever which occurred among the sailors was an aggravated form of the remittent fever. Considers the convalescence of Yellow Fever protracted. Considers a man recovers more rapidly, and is sooner equal for duty, after remittent fever than after Yellow Fever. Remittent is more subject to relapses than Yellow Fever. Thinks the spleen is more usually affected in the remittent than Yellow Fever. In remittent fever the vomiting is bilious. Does not recollect ever having seen black vomit ejected from the stomach in remittent fever.

In the early stages of both diseases the fluids vomited are nearly the same, with the exception that there is less bile in Yellow Fever. Has seen cases of remittent fever equally as severe in their symptoms as Yellow Fever, both in the Mediterranean and Demerara. Has not seen the fatality in remittent to be so great as in Yellow Fever. Never found black vomit in the stomach of any of the men who died of remittent. Saw from 30 to 40 cases in a detachment of the 33rd Regiment at Demerara in 1843—44, a lapse of four months. Cases were principally sent from one end of a particular barrack. Other barracks continued free. Were put into separate wards of the hospital, which received all fever cases. Several of the hospital servants were attacked with the same disease, but chiefly those employed as cooks.

Cannot recollect that any patient not in the fever wards was attacked. No medical officer attacked. In the deaths that occurred black vomit was found in the stomach. There may have been at one time 8 or 10 patients labouring under the disease in the hospital at the same period. Has known one man die of the disease, after he had been walking about and considered convalescent for 10 days. Black vomit was found in the stomach.

Considered this man on his admission a decided case of Yellow Fever. Considers the hospital servants took the disease from being occasionally exposed to a more unhealthy locality than the other patients who were in hospital, and accommodated in the higher parts of the building. Remittent fever was at this time prevalent in the garrison.

In 1844 thinks he had under his charge about 15 patients of the 23rd Regiment suffering from Yellow Fever. Nearly all were attacked about the same time (say over a month). The larger proportion of them was from the barrack which furnished the cases of the 33rd Regiment; still some of them were from other rooms. Does not recollect how many died. In all who did die, black vomit was found in the stomach.

Does not think it possible to mistake black vomit for dark bile. Hospital orderlies and cooks again attacked; also some patients in hospital: some of them died. From a detachment of artillery quartered about one-fourth of a mile from the Line Barracks, some cases were admitted into the Line Hospital (same).

At this time several cases of the Yellow Fever occurred among the sailors in the bay, and thinks some Portuguese, residents on shore, were also attacked. The epidemic of this year was considered a limited attack as regards numbers.

Several of the civil medical practitioners considered the disease a distinct one from remittent; others the reverse. There was no rumour in August, 1843, when the disease first appeared, that any vessel had arrived with any part of the crew labouring under the fever. No officer was attacked during the period the disease prevailed, though they used frequently to go round the hospital with the medical officers. Does not recollect any of the patients in the 23rd Regiment having relapses. Convalescence tedious. The cases that occurred in hospital occurred from time to time. In 1843 and 1844 one vessel each year (prizes) was brought to Demerara, but does not know whether slaves were on board. Troops suffered before the sailors. Cases of Yellow Fever with black vomit occurred at the same time at Berbice. Was very severe there. Two companies of the 33rd Regiment were at Berbice. Head-quarters at Demerara. First broke out at Berbice. Has never seen a remittent fever in the Mediterranean, accompanied by vibices, hæmorrhages, &c.

Thinks the same causes that will produce remittent in an old resident will produce Yellow Fever in new residents. Found the mucous coat of the stomach in fatal cases of remittent in the Mediterranean pulpy and denuded. With the addition of the black vomit, thinks the remittent in the Mediterranean would approach in appearance Yellow Fever. The morbid appearances found in the remittent fever of the Mediterranean resemble those in Yellow Fever, with the exception that the spleen is more

softened in the Mediterranean remittent. Has not met with any old residents in the West Indies attacked with Yellow Fever.

The prize vessels stated to have been brought in in 1843 and 1844 were in charge of small men-of-war. Does not recollect the crew of these vessels to have been sickly.

Steam communication once a-week between Demerara and Berbice. The 33rd Regiment had been about two years in the West Indies. The 23rd Regiment had just arrived. The strength of the 33rd and 23rd detachments were about the same. The barrack-room where the majority of cases occurred was immediately in the neighbourhood of a drain. The hospital was surrounded by drains. Rhubarb-coloured livers common in fatal cases of remittent in the Mediterranean. Same appearances usual in the liver of fatal cases of remittent in Demerara. Does not consider there is any difference in the colour of the liver in fatal cases of remittent and Yellow Fever. Considers any fever, remittent or otherwise, may, by undue crowding, bad ventilation, &c., produce a fever; whether the same or not is not prepared to say. At the time the patients in the hospital with other diseases were attacked with Yellow Fever, they were in another part of the hospital, but might have had communication with those ill with the fever, as no measures were adopted to prevent them, except caution.

Does not think he could, on admission, diagnose in cases between remittent and Yellow Fever; but thinks that before a patient with fever is 24 hours in the hospital he could decide whether it was a case of remittent or Yellow Fever. In remittent, before this period they would show a disposition to remit. In Yellow Fever, remissions, he considers, are not so defined. Did not observe remissions in any of the genuine cases of Yellow Fever that were admitted into the hospital among the sailors from the "Crocodile." Some of the cases sent to the hospital from camp were considered pure remittent, they having had distinct remissions.

Never saw black vomit or hæmorrhages in the severest form of remittent in Walcheren. Never knew a case of Yellow Fever terminate in intermittent. A frequent termination of remittent at Demerara in intermittent. Instances his own case.

Does not know, or has not heard, of any instance of Yellow Fever having originated in a ship whose hold is supposed to be in a filthy condition, or otherwise impure, in any seas, except those of the West Indies and Africa; but thinks he recollects that a naval surgeon mentions such an occurrence took place in the Mediterranean.

(Signed)

C. McLEAN,

*Deputy Inspector-General.*

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*Evidence of Dr. Dods, Surgeon, 36th Regiment.—October 18, 1849.*

Arrived in Demarara with two companies of the 88th Regiment in March, 1847; left it on 20th February 1849. Saw bilious remittent fever and intermittent during this period. Strength of detachment about 170. The two companies above mentioned were exactly 12 months in the command, and all, except six or seven, suffered from remittent or intermittent during that period; about three died. Attacks of remittent were most severe during August, September, and October.

Symptoms then were very high. Vomiting invariably present. Matters ejected from stomach of a dark green ropy appearance. Every officer in garrison suffered. Relapses almost invariable. Some cases of remittent ran into intermittent. Intermittent in some cases, unless checked, ran into remittent. Inhabitants suffered at the same time. Was considered to be the common epidemic fever of the country.

During the second year of my residence one sailor died of Yellow Fever, with black vomit, who had arrived from Barbados, where the disease was raging in the garrison. Was taken to the Seamen's Hospital. No case originated in the hospital after his admission. Believes that among the medical men of the colony the impression was that Yellow Fever and remittent fever were distinct diseases. Vessel was not put in quarantine.

Two companies of the 66th Regiment relieved the 88th in April, 1848. They left Barbados with head quarters of the 88th, who went on to Trinidad. Yellow Fever had been prevailing for some time in the 88th at Barbados. The 66th had never landed at Barbados, but had been sent on at once to Demerara on arriving at the West Indies. The 66th suffered greatly at Demerara from remittent fever and intermittent fever, much more severely than the 88th. A man had frequently from 8 to 12 separate attacks. Thinks not more than one of the 66th died from any fever. Vomiting was a constant symptom, and always bilious; tinged linen green. In the fatal cases, description of vomiting was the same. Every officer, except a surgeon, suffered from fever. Five officers were with the detachment.

The inhabitants suffered this year severely. Portuguese emigrants arrived during the year from Madeira. The Portuguese of the colony suffered very severely. The mortality amongst them was great. The mortality was occasioned by the common remittent of the country, not Yellow Fever.

The women suffered in proportion to the men, also pregnant women; but none of them died. Four under the latter circumstances suffered. Considers marsh malaria was the cause of the disease. Has seen patients, while under treatment for other dis-

cases, attacked with both remittent and intermittent; also hospital orderlies. Coolies from the East Indies suffered severely from remittent and intermittent. Mortality great among them.

Those who had not regular occupation, or cared for, died in great numbers.

Convalescence after attacks among soldiers was protracted.

Yellowness of the skin occurred in most of the cases during the progress of the disease, but was most marked in second or third attacks.

(Signed)

GEO. DOUGLAS DODS, M.D.,

*Surgeon, 36th Regiment.*

*Evidence of Surgeon Parry, Half-pay, 4th Foot.—*

*October 31st, 1849.*

Served in the West Indies between eight and nine years. Has seen Yellow Fever during that period on two occasions, once at Antigua in autumn of 1817, and the second time at Montserrat in 1821. Prevailed at Antigua as an epidemic, but to limited extent at Montserrat. Was in charge of left wing of 73rd at Antigua, four companies strong. At Montserrat was in charge of a detachment of 35th Regiment. Early in autumn, at Antigua, the 63rd suffered from a mild form of bilious remittent, which became aggravated as the season advanced; about 15 deaths occurred, the majority with black vomit. All the men that suffered had been employed on duty at the dockyard at English harbour, and no one was attacked who had not been employed on that duty. Is not aware that any fever existed at English harbour at the time among the inhabitants there, or persons employed at the dockyard. Think there were about 50 military who had no communication with the dockyard, and those all escaped. Most of the officers who were on duty at the dockyard from time to time were attacked. One officer, a new comer, died with black vomit; ascribes the cause of this disease to malaria. Very few people at this time were employed at work at the dockyard; only the boatswain and a few blacks lived there. States there were not more than three or four whites living in the town; the inhabitants were chiefly coloured people. Not a single instance of a person in hospital being attacked occurred; none of the attendants of the sick officers were attacked. Considers the disease in question was an aggravated form of the common remittent fever. Considers an aggravated form to be a disease showing more severe symptoms and being more fatal in the result. Has seen a remittent fever at Guadaloupe, with symptoms equally severe and of nearly equal mortality, without black vomit being so generally present. He

thinks it occurred (black vomit) in two cases. Has often seen remittent fever in other stations in the West Indies occurring without black vomit ever taking place, and many of the cases terminated fatally. Saw no circumstances on any of those occasions that would have led him to believe that one or the other disease might not have prevailed at the station.

Believes there are different kinds of fevers in Europe, and that these fevers depend upon different causes; thinks that there are different kinds of fevers in the West Indies, arising from different causes.

Thinks in the West Indies there is simple continued fever, the result of trifling irregularities; secondly, the remittent fever. Considers these two embrace all kinds, and states he never saw any fever there which he considers contagious, or bearing any analogy to the typhus of Europe. Has never known ulceration of the bowels in Yellow Fever. Considers this ulceration of bowels a characteristic symptom in typhus. Tongue in Yellow Fever is generally clean, and teeth seldom incrustated with sordes. Livid blotches on the skin in the fever at Antigua did not occur; but yellowness of skin was a frequent symptom. Vomiting first was contents of the stomach, with bile; then (say on second day) a glairy fluid would be ejected, the general precursor of black vomit. Those that died with black vomit generally expired between the third and fifth day. Relapses were frequent; convalescence was very slow. Few died from the first attack; the fatal cases were generally relapses.

No marshes in the neighbourhood of English harbour; no fresh troops had joined the detachment for some time previous to the outbreak. Dockyard is considered to be a very unhealthy place.

Disease appeared at Montserrat, in the town, in the person of a young lady who was born in the island. She died with black vomit. In the same street, midway between these, a young mulatto boy lived; he died with black vomit. This he thinks was the second case. About 100 yards distant in the same street a Commissary and his family lived; one of his daughters was about the fifth person who was attacked; she died with black vomit. In a few days after her death the Commissary was attacked, and died in a few days with black vomit. The remainder of the family was removed from the town to the barracks in about ten days after his death, and on the day after removal the youngest daughter was attacked, but recovered. They were placed in a house about 200 yards from the barracks. The remainder of the family (four in number) escaped the disease. Prior to the elder daughter being attacked, three of the black servants were attacked with fever; they occupied the ground-floor. About ten days after the first case in town the disease appeared in the barracks, but it was of short duration, and only one man died.

The family of the young lady first attacked consisted of father, mother, and five children; all the children were born in the island, also the father; the mother was a native of England. Thinks the number of cases among the troops was four or five; all these cases were men. They were frequently in the town; none in hospital were attacked. All were men at their duty.

One of these cases was the barrack-sergeant who died; black vomit appeared previous to death. He had undergone great fatigue immediately before the attack. States black vomit to be a brown fluid, with black flakes in it, and vomited without any effort. Would not have called it black vomit had black flakes not been present. Has never seen in any other fevers a dark fluid ejected from the stomach which bears any resemblance to black vomit. When he arrived in the West Indies he was under an impression, from what he had heard and read, that remittent fever and Yellow Fever were one and the same disease, and the opinion was afterwards confirmed from observation. Has seen remittent fever prevailing to considerable extent in localities where he would not, *à priori*, expect marsh miasmata to exist.

At Bellary, East Indies, remittent fever of a most severe form is frequent, and in it, remissions are always very marked, and amounting almost to apyrexia, which fever he imputes to marsh miasma. This fever never terminated in black vomit; in the greater proportion of cases there was no vomiting; in some it occurred. When it did occur, the contents of the stomach and bilious fluid were the matter ejected. The cases that occurred at a certain period of this epidemic were as severe in their symptoms and nearly as fatal as the cases of fever that occurred at Antigua in the 63rd, above alluded to. Those who did not die suffered much from relapses. Considers this the same disease as the remittent fever of the West Indies. Has never seen remittent fever during his service in New South Wales. Never saw remittent fever terminate in intermittent in the West Indies, but often in the East Indies.

In cases of Yellow Fever, liver was often of fawn colour, and enlarged sometimes; spleen in some cases considerably enlarged. Mucous coat of the stomach generally about the great curvature, and especially about the pylorus, detached.

Does not recollect that remissions occurred in the severe form of fever, with black vomit, at Antigua. Considers the fevers of Bellary and the West Indies only differ from each in the organs implicated, that is, the principal lesion in the East being the brain, and the chylopoetic viscera in the West. Where the brain is much affected in the fevers of the West Indies, there is no black vomit.

Has seen yellowness of the skin in the fevers of the East Indies, but is not a common symptom.

Disease of the spleen never follows an attack of remittent fever at Bellary.

In fatal cases of ordinary remittent, has not observed the morbid lesions to differ materially from those found where the termination has been preceded by black vomit. At other times within his experience, when the guard was mounting at the dockyard, cases of fever were admitted from time to time, but none had black vomit.

Did not separate his fever cases from others in hospital. If black vomit does not occur during an epidemic, it is called remittent; if it does, it is called Yellow Fever.

In epidemic Yellow Fever the powers of life seem often completely overcome, the patients often having a staggering gait, a tremulous tongue, with a very weak pulse. In remittent the invasion is generally indicated by higher symptoms, the pulse being strong and full.

Yellow Fever prevailed at St. John's, Antigua, at the same time that it prevailed at English harbour.

(Signed) W. PARRY, *Surgeon, Half-pay.*

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*Evidence of Dr. Kelly, Surgeon, Ordnance Medical Department.—  
November 8th, 1849.*

Served in Jamaica between November, 1835, and February, 1838. Saw Yellow Fever during this period on more than one occasion both in the Royal Artillery and line. Stationed at the time at Port Royal. Saw it also frequently in the Royal Naval Hospital at Port Royal. Considers it a different disease to the usual remittent fever of the island; thinks he could discriminate, at the commencement of the attack, whether the disease was remittent or Yellow Fever.

In Yellow Fever there is a peculiar aspect of the patient,—great prostration and an unwillingness on the part of the individual to describe his actual state. In remittent fever the symptoms are more developed; there is decided pain in the head, and the patient shows less aversion to explain his condition. In remittent fever there is generally greater excitement after the invasion of the attack than in Yellow Fever. In Yellow Fever, terminating fatally, the yellowness of the skin was usually concomitant with the black vomit, not before. Yellow Fever generally terminated fatally about the fifth day; occasionally protracted to the eighth day. Black vomit appeared to be a fluid resembling flocculi, suspended in a dark-coloured serum,—the former, when left to settle, falling to the bottom. This was the character of the fluid which I denominate black vomit.

Has never seen any description of dark fluid vomited from the stomach in remittent fever. Thinks black vomit sediment to consist of effused blood. Thinks the evacuations from the bowels in Yellow Fever first consist of biliary fluids and fluids affected in colour by medicines, &c. Never knew a person attacked a second time with Yellow Fever. The disease generally appeared in the morning most frequently after the patient had been on guard at night, or when exposed to low temperature; considers this the general cause of the disease. Saw no case which he could have considered to have been produced by any virus emitted from a patient labouring under the disease. Average number of sick in hospital from 15 to 20. Never knew any of the sick in hospital with other diseases attacked with Yellow Fever. Never knew any of the hospital servants attacked. Had two cases of officers attacked with the disease; they had coloured servants. Servants not attacked.

At this time nine officers were present with the detachments.\* The number of patients in Naval Hospital varied usually from 35 to 40; might have seen from 35 to 40 cases of Yellow Fever in the Naval Hospital during his period of service at Port Royal; cases there were generally of a serious character.

Does not think there is any infectious fever in Jamaica. There is considerable resemblance in the mode of invasion and progress of symptoms of Yellow Fever to the typhus gravior of Europe. This resemblance does not exist with respect to remittent fever; but thinks remittent fever may pass into a typhoid form, and saw a lady in which he considers this happened. She fell into that state after being considered convalescent, and died without black vomit. Thinks the evidence of the non-contagious nature of Yellow Fever was sufficiently strong to render it unnecessary to separate those with that disease from unaffected persons. Classified his patients according as they presented one or more of the series of symptoms above stated. Thus he had at times cases of remittent fever and Yellow Fever admitted within a few days of each other. None of the cases classified as Yellow Fever had relapses during convalescence; some of those with remittent fever had relapses. Thinks the men who suffered from Yellow Fever had been nearly two years in the company; five of those who suffered from remittent fever (three of whom died) men who had been sent there for duty, were only lately from England.

In fatal cases of Yellow Fever, liver was generally of straw colour; otherwise not much affected. Spleen generally enlarged and soft; inner coat of stomach commonly very soft. As regards structure, somewhat like wet blotting paper. Near the pyloric

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\* Detachments at Port Royal consisted of 1 company of Line and 2 officers 2 companies of Artillery with 7 officers.

orifice usually vascular; sometimes in patches, as if intestinal effusion had taken place. Black vomit was found in the stomach of one who died without having had any previous vomiting, and generally in the other fatal cases; sometimes in only very small quantity.

In fatal cases of remittent fever, liver was pale; not otherwise affected. Spleen enlarged, and usual colour. In no case was black vomit found in the stomach. Considers the symptoms of Yellow Fever owing to the greater depression of the vital powers, and to constitute a disease of more aggravated nature than remittent fever; the proportion of deaths was also greater. Observed diminution of the severity of the symptoms in Yellow Fever, but not to amount to what he considers a remission. In remittent fever there was a considerable diminution of the symptoms at certain times, followed by a marked exacerbation.

Served at Beyrout, in Syria, between July 40 and March, 1841; at which time was stationed there a company of Royal Artillery and half a company of Sappers and Miners. Some of them suffered from slight remittent fever; remissions distinct. Saw an artist at Jerusalem die of pure remittent fever. No cases of relapses among the soldiers. Skin was not tinged yellow in these cases.

During the three years he was stationed at Port Royal, Yellow Fever never occurred as an epidemic, but the cases were sporadic, and occurring now and then. Considers the cause of remittent and intermittent fever to be marsh miasma.

During the period the navy was much reduced, the "Comus," he believes, had several cases of Yellow Fever. Considers that the remissions of fatal cases of remittent fever were more marked and distinct than in the fatal cases of Yellow Fever. With the exception of the black vomit and softening of the mucous membrane of the stomach and intestines, there are no great morbid differences between the two diseases. The duration of fatal cases of remittent fever far exceeded those of Yellow Fever. No cases of Yellow Fever terminated in remittent fever; no hæmorrhages from gums, &c., occurred in remittent fever. Irritability of stomach was greatest in remittent fever, in the early stages. The vomiting was bilious in remittent fever; the vomiting in Yellow Fever was not accompanied by biliary discharge, but was mucus. The vomiting in fatal cases of remittent fever ceased some time before death; the reverse took place in Yellow Fever.

(Signed) WILLIAM KELLY, *Surgeon, O.M.D.*

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*Evidence of Dr. Daniell.—January 9th, 1850.*

How long did you reside on the western coast of Africa, and within what latitudes?—About nine or ten years; and I resided between  $8^{\circ}$  north and  $14^{\circ}$  south of the line.

Did you see much of fever during that period? What were the kinds of fever, and did it affect all in common, or were some confined to blacks and some to whites?—Saw a great deal of fever during that period, namely, intermittent, remittent, ardent or continued, and Yellow Fever. All these fevers were confined to whites.

What was the description of white persons that suffered from these fevers?—Traders, resident on shore and board of ship, crews of vessels, and missionaries.

On what occasion did you see Yellow Fever?—On board a Danish vessel bound from Sierra Leone to the Gold Coast, near Cape Palmas (believes in 1838). At this time I was on board a vessel: I went on board this vessel at the request of the captain of the Danish vessel, where I found two men suffering from severe fever, stated to have been of continued type. I found that four or five had died of fever previous to this period, and was informed that in all these cases they had vomited some black fluid, and I saw in a basin some of that which had been vomited by the last case that died.

Did you recognize that fluid as similar to any fluid that you had seen vomited in any case of fever that had come under your observation on the coast?—No; I never before had seen any fluid like it. The skin, in the two cases I saw, was of a tawny hue as far as I could discover, the patients being in an obscure part of the vessel. They had been sick when I first saw them two or three days. Saw them again the same day, prescribed for them, and then the vessels parted. The captain and crew were all Danes. I continued in good health after having visited these men. I believe her crew consisted of about thirteen men. No others but the four who died had been attacked during the voyage. The captain or mate stated to me that he had had the fever in Sierra Leone, and that therefore he was not afraid of being attacked. The captain of an English vessel subsequently informed me (which had arrived at Fernando Po from the Gold Coast) that the vessel above referred to had lost all of her crew except two or three, from this fever.

The ardent fever I saw on the African coast was a continued fever, without remissions, of an inflammatory nature. Sailors were the most frequent subjects of this fever. The resident traders suffered but little from this fever; they suffered chiefly from remittent fever or intermittent. The residents on shore, who suffered principally from remittent or intermittent, had frequently in the same year second attacks of remittent, or sometimes one

attack of remittent and one of intermittent. I had three attacks of remittent fever in one year, in the Congou province.

In the low type of remittent fever, the mortality was at least eight or nine out of ten attacked.

On one occasion, at Rio Forinosa, three vessels (their crews consisting of about sixty whites in number) lost the whole number, and having obtained second white crews, lost, I think, about one-half. This occurred within a period of six months, between August and February. The sickly season in that locality is between August and October inclusive.

In a vessel in which I was, out of a crew consisting of nineteen whites, including myself, six died; all suffered from the disease except one or two.

The disease in the early stage appeared mild; commonly about the third day it became aggravated, and then remissions of a few hours commenced. The remissions were not preceded by regular rigors, but by a sense of cold. I saw and treated the whole of the cases on board the vessel on which I was, and also a good number on board the other vessels. Vomiting of bile and retching was a common symptom of these fevers in the early stages. The remissions consisted of a marked diminution of the heat, with a slight tendency to perspiration; pulse soft and almost natural; tongue moist; the patient felt free from oppression and expressed himself as feeling much better. In no instance of a fatal case, or any case, did I see any fluid vomited from the stomach bear the least resemblance to the fluid which was shown to me as having been ejected from the stomach of the man referred to as having died on board the Danish vessel. I never saw any fluid in these fevers vomited which did not appear to me to be bile. Tongue was frequently black; at other times morbidly red and dry; at other times the tip and edges red. In the protracted cases, the teeth were frequently covered with sordes. The alvine evacuations were sometimes dark, sometimes natural; in the early stages dark sometimes, becoming lighter as the disease advanced. Bile was distinct in the evacuations towards the conclusion of the disease; discharges of blood were frequent from the gums, nose, and anus. These discharges from the nose were sometimes profuse and sometimes small; the same from the gums: in a few cases there were discharges of blood from the ears. After these discharges, generally speaking, the patients recovered; relapses frequent; convalescence protracted. (Sometimes there were discharges of blood from the inner side of the eyelids, in the most fatal cases.)

I opened the bodies of a great many cases which died. The mucous membrane of the stomach in some was congested, either generally or in patches, particularly about the pyloric orifice. In no case did I find black vomit or any dark fluid; found nothing but medicine and the natural secretions of the stomach.

The colour of the liver was rather paler than natural; some

times larger than natural; sometimes the ducts engorged with bile; gall-bladder generally full of bile; spleen frequently enlarged and friable, to wit, easily broken down; blood never thoroughly coagulated when taken from the arm during life. In some cases there was suppression of urine, but not generally. In the bladder, after death, there was generally a small quantity of urine. Death, when it occurred, was from the seventh to the thirteenth day. I cannot say exactly, but I think I have opened about 100 bodies. I frequently saw remittent fever terminate in intermittent.

Is not sure whether he ever saw intermittent run into remittent fever.

After recovery from remittent fever, some recovered without any visceral disease whatever; others after recovery had enlargement of the spleen.

In my own case, I had an attack of remittent fever, and recovered perfectly. I then had a second attack, and it left disease of the spleen.

With reference to my observation, taken down by Dr. Burrell,—“Never saw any one suffering from visceral disease after these attacks,”—I meant disease of large or small intestines. Of those that remained under my own observation for some considerable time after they had recovered, I think (but I am not certain) about one-third had disease of the spleen as a sequel.

I saw remittent fever in Benguela, Angola, Congo district, Gaboon River, Fernando Po, Camaroons River, Old Calabar Bonny River, New Calabar River, Brass River, up the Niger, Rio Formosa, Lagos, Weeda, Slave Coast, Gold Coast, Windward Coast, at the Gambia, and Sierra Leone.

When I went on board the Danish ship, I was there altogether during the two visits about an hour and a half, but I did not contract the fever. About two months previously I had a fever at Sierra Leone, which I was informed by Dr. Ferguson, who attended me, was Yellow Fever. I had been on shore at Sierra Leone from Liverpool about two weeks before I was attacked. I did not see any cases of Yellow Fever at this time.

I do not consider that black vomit should always be present in Yellow Fever.

The duration of ardent fever was from seven to fifteen days.

In the low remittent fever there were always remissions. Succeeding the remission on the third day was an exacerbation.

None of the crew of my own vessels were attacked with remittent fever while at sea; but on getting within the rivers, close to shore, or going on shore, they were attacked.

Never heard of Yellow Fever appearing in any vessel on the coast which had arrived with her crew in a healthy condition. No other person in the house I lived in, as far as I know, suffered from the fever under which I laboured at Sierra Leone, either before

or after the attack; nor had I, as far as I know, been near anybody with the fever. I suffered in June.

I was in the constant habit of going about the town.

In the remittent fever the skin was generally yellowish.

(Signed)

WILLIAM F. DANIELL, M.D.  
*Staff Assistant-Surgeon.*

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*Evidence of Dr. Hawkey, Staff Surgeon, 1st Class.  
January 10th, 1850.*

Served in Jamaica, St. Lucia, and Barbados, as a Staff Surgeon, first class, altogether nearly six years. Occasionally saw during this period ephemeral fevers, from usual irregularities, without general or local yellowness of skin. On all occasions when I witnessed Yellow Fever prevailing at Jamaica (where I only saw Yellow Fever prevalent to any great extent), there also prevailed the common remittent fever. I consider a fever to be remittent fever when there are periodical abatements of the violence of the symptoms, lasting for a longer or shorter period.

I have never seen any case of fever in the West Indies which I consider a continued fever. So far as my experience goes, the fevers I saw in the West Indies, the result of irregularities, presented remissions. In many instances I saw fevers, which were extremely mild in their commencement, terminate in Yellow Fever; but I considered these fevers Yellow Fevers from the commencement.

In Yellow Fever it was difficult to decide whether the abatement of the symptoms amounted to a remission; in ordinary remittent fever the remissions were distinct. Observed, during the prevalence of Yellow Fever, cases of remittent fever prove fatal without the disease changing its character. Yellow Fever was in many cases mild at the commencement, and, when it became aggravated, an abatement of the symptoms was observed in some cases; others terminated fatally, without any abatement of the symptoms. I have seen cases of what I consider genuine remittent fever, take on during their progress the character of Yellow Fever.

The liver was of a nutmeg colour, in the fatal cases from remittent fever.

The mucous membrane of the stomach was thickened in some cases; in others, abraded and softened. Never saw black vomit in the stomach of such cases. Does not recollect any peculiarity of the spleen. In Yellow Fever the morbid appearances were much the same, with the exception that black vomit was contained in the stomachs of most of them.

In Yellow Fever at Jamaica, when death ensued, it generally took place between the third and seventh day.

I have seen persons suffer at least twice from the regular remittent fever.

I think I recollect persons suffering from Yellow Fever who had suffered from remittent fever on some previous occasion.

I consider the cause of remittent fever to be some exhalation from the earth, at a time when a peculiar state of the atmosphere prevails. From what I have heard and read, I believe Yellow Fever depends upon the same cause.

I believe that in a small room, with want of proper ventilation, and crowded with persons labouring under Yellow Fever in an aggravated form, that a healthy person, breathing the atmosphere of the room for some time, might contract the disease (Yellow Fever).

I consider that under such circumstances the atmosphere is morbidly polluted by the bodies of the sick, which pollution creates the same disease as that under which the sick laboured.

I consider Yellow Fever, remittent fever, and intermittent fever, only grades of one and the same disease.

I have seen cases of Yellow Fever die, without any traces of black vomit.

I know of no disease, except under the general term of contagious diseases, that give an immunity from second attacks, except in rare instances.

I have never seen intermittent fevers (the lowest grade of Yellow Fever, according to my opinion) arise in Barbados among white persons.

I cannot account why the aggravated form should occur when the lower form did not occur. My experience of Barbados is confined to a few months.

In mild cases, when no epidemic Yellow Fever prevails, I think I could diagnose a case of Yellow Fever, if it were to occur. I would chiefly depend upon the peculiar expression of the countenance, and the hollowness of the eyes.

If a man should come into hospital after a debauch, with headache, hot skin, vomiting, suffused eyes, and pains in the bones, I think I could distinguish this case from Yellow Fever. The expression of the countenance of such a man would be very different from that of a man labouring under Yellow Fever. But there are cases of Yellow Fever where this peculiarity does not occur at the onset.

(Signed)

WM. HAWKEY, M.D.  
*Staff Surgeon.*

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*Evidence of Surgeon Bradford, 1st Battalion 23rd Fusileers,  
January 11th, 1850.*

I served in Jamaica, as a regimental medical officer, during five years and seven months, and in Barbados, Trinidad, and Antigua

for three years and five months. During these periods I saw Yellow Fever on a large scale, on many occasions. Fever prevailed, during these periods, in the form of intermittent and remittent, and the aggravated form of the latter, viz., Yellow Fever. I also saw fevers arising from irregularity. These latter were of a continued type. All the fevers, except the latter, that I saw, exhibited either remissions or a tendency to them, except in cases of extreme intensity.

I would characterize remission to be an abatement of heat, of general suffering, of delirium, and a diminution of the force and frequency of the pulse, lasting from three to twelve hours.

In severe cases of the aggravated form, or Yellow Fever, if the patient lived for three or four days after admission, it has seldom or never occurred that I have not observed at intervals a remission, as above described, more or less complete.

In warm climates, the abatement of symptoms is much more remarkable than in the fevers of cold climates; the sinking, cold extremities, and cold clammy sweats, which are so apt to appear in the fevers of hot climates, and even in the most robust frames, are remarkably distinctive from the abatement of symptoms which occurs in the fevers of cold climates. After the abatement of symptoms above described in warm climates, the symptoms of excitement again take place. I have frequently heard patients, during the abatement above described, express themselves so much relieved as to deny they were ill.

I have often seen what is styled remittent fever prevailing, when the aggravated form, called Yellow Fever, did not occur. Some of these cases proved fatal, though rarely without exhibiting the symptoms of the aggravated form above described.

The morbid appearances of these cases were—the vessels of the stomach congested; the mucous coat but little changed; the stomach found to contain the ordinary fluids recently swallowed, or secreted, slightly tinged with bile; the liver appeared to contain more blood than in its natural state, its structure and size unchanged, nor have I observed any marked change in its colour; bile in the gall-bladder has become like the viscid pulp of some fruit; spleen, generally speaking, was softer, from its containing more blood, and was, therefore, easily broken.

I think I have examined eight or ten such cases, and seen many others examined. In all of them the above were the morbid appearances. I have often seen cases of what is called remittent fever terminate in intermittent. I have also seen cases of intermittent terminate in remittent. I have seen cases of intermittent fever terminate fatally. In these cases the morbid appearances after death were similar to the cases of remittent, above described. In intermittent fever I have seen sallowness of skin, but not amounting to intense yellowness. In the remittent fever the sallowness was not so common as in intermittent, but I have seen

it in both, but not the intense yellow that occurs in the aggravated form of remittent fever (Yellow Fever).

I have seldom seen organic visceral complaints follow fevers in the West Indies. In the ordinary remittent, convalescence was comparatively rapid. The morbid appearances in the aggravated form (Yellow Fever), were intense congestion of the vessels of the stomach, duodenum, and even jejunum, causing their internal coat to appear of a deep crimson colour; mucous coat softened, but not from the effect of the disease; the congestion was chiefly at the cardiac extremity; no appearance of abrasion or loss of substance of any kind. In the majority of cases, the contents were similar to those I have described as being found in fatal cases of remittent fever; but in about a fourth there was an inky fluid, constituting the black vomit; this fluid I have seen in duodenum and jejunum. I have found as much as a pint or a pint and a half, but generally the quantity was smaller. The black vomit was a uniform fluid, like a mixture of ink or soot and water, but I never minutely examined it, to ascertain its exact nature. It remained, after standing, of a uniform colour. The liver contained more blood than when in a healthy state, more particularly the branches of the vena portæ. Its structure was not materially changed; its size was generally rather increased; its surface colour not altered materially, nor when cut into did I observe any alteration in colour. I have seldom seen a case of fever admitted into hospital which was considered by me, at the period of admission and for some days afterwards, the ordinary remittent, assume subsequently the symptoms of the aggravated form (Yellow Fever).

The cases of the ordinary remittent fever proved fatal most frequently on the ninth, tenth, or eleventh day; in the cases of the aggravated form, death occurred generally on the fourth, fifth, or sixth day. During epidemics, the cases of the aggravated form (Yellow Fever) were generally admitted from certain localities of barracks, such as the portion occupied by a particular company, or a particular building, as in the case of officers and married people. At such times, the fever cases admitted from other localities exhibited the character of the ordinary remittent. I never could detect any reason, either in the locality itself, or in the state of matters around it, to account for this difference.

The married people were generally located in inferior buildings; these buildings were commonly huts within the enclosure; the surface drains ran nearer to them, but I never saw anything offensive in these drains. Thinks the rooms were generally crowded, and not so well ventilated during the night as the ordinary barrack rooms. The severe cases among the soldiers came generally from the lower floor, which is situated on arches, on an average, elevated at least 4 feet from the ground. I think the huts furnished a greater proportion of cases of the disease than the lower barrack rooms. The officers who suffered, within my know

ledge, in Jamaica, were generally living in upper rooms; the worst cases (three) among them came from one of the field officers' quarters at Up Park Camp. In this quarter there were no open drains, no want of ventilation, or any assignable reason why the disease should have occurred. The same applies to the quarters of the other officers.

I consider the disease was caused by a local vitiation of the air. The severe cases arose in different parts of the barracks at the same time. I never believed, from my own observation, that the disease was propagated from one person to another. I did not observe that when once it appeared in a barrack room it spread to any great extent in that room.

At Up Park Camp, while the disease prevailed there in 1832, out of 555 men, 319 were attacked, and 43 died; of these 43 the whole did not die from the aggravated form, but the majority did. The aggravated form appeared, chiefly, in July and August.

After the disease had ceased in the portion of the 56th Regiment at Up Park Camp, a company of the regiment was brought into camp from Kingston, and was attacked with the disease (about one-fourth of the number); many of them died, and the disease was of an aggravated form.

The disease in an aggravated form at Up Park Camp ceased in August, and the company arrived from Kingston in either the last week in November or first week in December. In this interval some ordinary cases of remittent occurred at Up Park Camp.

I have frequently seen repeated attacks of the ordinary remittent in the same individual; I have seen the same as regards intermittent, but more frequently than in remittent. I have known persons suffer from the ordinary remittent who have suffered from the aggravated form (Yellow Fever). I have known persons suffer from the aggravated form of the disease a second time, with symptoms similar to the first attack. I know two such instances—Hunt, a musician, and Houtoway. With reference to Hunt, his first attack occurred in July or August, the other late in the year (say December); it was during the time when the company from Kingston was suffering from disease.

I think persons who had suffered from the malignant form were very much less liable to second attacks of the malignant form than those who had suffered from ordinary remittent were to second attacks of the latter disease.

At the time the disease, in its aggravated form, commenced in 1832, several patients, who had been admitted into hospital prior to the disease appearing in the barracks (one I may mention, a fractured clavicle), were attacked with the fever in an aggravated form, and died. At this time the sick list was about 26. Two of the hospital attendants, at least, contracted the same disease, and died of it. There were three medical officers doing duty; none of

them were attacked. None of the officers (in 1832) nor their families suffered from any description of fever. I have seen intermittent and remittent fever occur in situations where there was no swamp; but these diseases are more common where there are swamps. At Falmouth, which has swamps on three sides, intermittent and remittent were rare. I was there a year.

The swamp is partly supplied by sea, and partly by rivers or water from the surface of the earth. I never observed intermittent to originate at Up Park Camp.

I consider the reason why we did not experience the lowest grade of fever, viz., intermittent, was that the cause was sufficiently powerful to produce the more aggravated forms; and I account for the ordinary remittent and the very aggravated form (Yellow Fever) having both occurred at the time, to the circumstance of the different susceptibility of individuals. I ascribe the complete exemption of some to the same circumstance—a lesser susceptibility.

During the existence of the epidemic of 1832, cases of fever were admitted arising from ordinary irregularities, and designated as common continued fever. Some of those cases, while in hospital, assumed the aggravated form.

I was at Trinidad (1844, 1845, and 1846) for 15 months, with three companies, during this period. Saw no case of the aggravated form (Yellow Fever). I think about 130 or 140 cases occurred of the ordinary remittent.

I was in Barbados first 13 months, and subsequently three months, in 1844 and 1847. In the month of August, 1844, a mild remittent prevailed, with decided remissions. All the cases, men and officers, showed a tendency to remit. Had no case of intermittent during this period, except those brought from Demerara. I am of opinion the cause that existed there was not sufficient to produce the aggravated form; but I cannot account why it did not produce the intermittent.

In fatal cases, after a first attack of the ordinary remittent, I have not observed disease of the spleen; but my attention was not specially directed to the inquiry.

I do not know any diagnostic mark between ordinary remittent and aggravated remittent (Yellow Fever), before a certain stage of the disease.

I very rarely saw black vomit ejected from the stomach during life.

I consider the vomiting in Yellow Fever is greater in the early state than in remittent. The yellowness of skin in Yellow Fever is deeper than in remittent. I have never seen passive hæmorrhage in the ordinary remittent, but I have in the aggravated grade (Yellow Fever).

I was quartered at Chappleton, Jamaica, in July 1833, with a detachment of 90 men and 4 officers, with some white women and

children and black servants. Fever of the most malignant remittent form, without black vomit in any case before death, and with occasional deep intense yellowness of skin and marked remissions, broke out about three weeks after arrival, and attacked every individual except one. Fourteen men and one woman died between  $4\frac{1}{2}$  months: the party was then removed. No marshes existed in the neighbourhood, but it rained at least 83 days.

No idiopathic case of intermittent occurred, but it was a frequent sequel of remittent. The civil population suffered all round us. Several managers of estates in the neighbourhood died. The civilians considered such an occurrence as a fever of this description uncommon. The black servants of the officers suffered from the same disease. Several had severe attacks, but none died. When relapses occurred, they were of intermittent form.

The fever I saw there was as severe as the malignant form of fever at Up Park Camp, with the exceptions that the stomach was not so excessively irritable, and that black vomit did not occur.

On the removal of the detachment to Port Royal, the disease ceased immediately. The company that relieved us at Chappleton was attacked, but removed before any serious consequences ensued.

Chappleton is distant from the nearest point of the sea-coast about 18 or 20 miles, and I suppose it to be from 1,000 to 1,200 feet above the level of the sea.

(Signed)

EDW. BRADFORD,  
*Surgeon, 23rd Fusiliers.*

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*Evidence of Staff-Surgeon (2nd Class) Connell.—January 14, 1850.*

I served in the Windward and Leeward Islands seven years, from October 1841, to September 1848. During this period I was in Dominica from December 1841 to April 1844. After that I was about two years in Demerara, and eight months in Barbados, 1846; then in Grenada from September 1846 to September 1848.

During this period I had an opportunity of seeing intermittent, remittent, and malignant-remittent, or Yellow Fever, at all of these stations. By a remission I mean an abatement of the fever, the vascular action reduced, head-ache less, heat reduced, and the patient expressing himself as feeling more comfortable.

These remissions vary in duration. I would call the abatements of the common continued fevers in this country remissions, as they resemble the remissions of the fevers in the West Indies. I think I have seen about 10 or 12 cases of the malignant remittent fever prove fatal, and had an opportunity of examining them after death. I found black vomit generally in the stomachs of these cases. The mucous membrane was generally softened. The livers on

convex surface were sometimes of natural colour, and sometimes of mottled marbled appearance, like blue and white or Castile soap.

I think during the year 1844, at Demerara, I saw three or four deaths from malignant remittent fever in one week, out of a strength of about 19 whites. I saw a case of fever in Barbados in 1846, in a man of the 7th Fusileers, which I considered was Yellow Fever—it was treated as simple continued fever. Assistant-Surgeon Collings treated the case, and watched the case closely, but could not discover any remission; so far as the symptoms were concerned, it appeared to me a continued fever. The fever had existed about two days when I saw the patient. He died the same night or the next morning. Had no vomiting before death of any kind, nor was the skin yellow. I was present at the post-mortem, and black vomit was found in the stomach after death. Dr. Davy was present, and desired that the black vomit should be tested; the quantity was about half a pint; colour was like that of coffee-grounds. There was no other similar case at that time. I consider that calomel and quinine are equally efficacious as a remedial agent in the malignant remittent as in the ordinary remittent. I am of opinion that I could distinguish the remittent fever from all other fevers by a peculiar heat of skin; by other fevers I mean continued and intermittent.

I consider in 1847 an epidemic remittent fever prevailed at Grenada when I was there. It was a common occurrence for remittent fever to terminate in intermittent. I never saw intermittent fever take on the remittent form of fever. I consider all the cases of idiopathic fever that I saw in the West Indies, whether from the usual irregularities of soldiers or otherwise, were remittent fevers.

In all my practice of remittent fever from the period of Dr. M'Lean's illness, I adopted one uniform treatment. I used the same treatment, viz., calomel and quinine, in cases coming into hospital from the usual irregularities of soldiers. I consider Dr. M'Lean's case was the ordinary remittent. In the severe remittent, remissions and exacerbations in many cases occurred clearly; in others the disease appeared to cease with the first remission. In the fatal cases of malignant remittent fever, the remissions were generally less distinct than in others.

I have seen many cases where I could not see a remission. In the malignant fatal cases of remittent fever, death usually occurred on an average about the third day. In all the fatal cases that I saw, amounting to about 12, the remissions were very indistinct, if at all manifested.

I have not noticed disease of the liver or spleen to be a consequence of attacks of remittent fever.

I have never seen an instance of a second attack of the malignant form of remittent fever. From the ordinary remittent I have seen second attacks in the same individual, or even a third.

I do not think the first attack of remittent fever increases the susceptibility to subsequent attacks.

Convalescence from malignant remittent fever was slow. In the ordinary remittent fever convalescence was quick.

I have not seen any man who had suffered several attacks of the ordinary remittent, suffer subsequently from the malignant form of it; it is not uncommon for a man who has suffered from the malignant form to have an attack of the intermittent or ordinary remittent. It was an opinion generally prevalent in the West Indies among the non-professional people, that one attack of Yellow Fever was a kind of protection from a second attack.

I do not recollect ever having had any private conversation with medical men on this subject. I never knew any soldier or civilian having had a second attack of Yellow Fever. As far as my observation goes, the white Creoles suffered from remittent and intermittent fever. The black soldiers suffered principally from common continued fever, and occasionally ague. I recollect having seen one case of epistaxis in a case of remittent fever; I heard of some others. These cases recovered. The remissions did not take place at any particular hour of the day.

I consider simple residence in the West Indies lessens susceptibility to fever of any kind. Never knew a case of black vomit but in Europeans. Remittent fever is a very rare occurrence in an African soldier. I think I know of one case.

I do not recollect having seen in any of the cases that proved fatal, passive hæmorrhages.

I have known patients or orderlies attacked in hospital with severe remittent fever, after cases with that disease had been admitted into hospital. This took place at the time when the three patients above referred to died in one week. One of the three that died was a man in attendance on the other two, sent for that particular purpose.

I do not recollect any of the other patients or orderlies were attacked at that time. The three men who died were artillery men, and we had scarcely any persons in hospital at the time but blacks.

(Signed) JAMES CONNELL,  
*Staff Surgeon, 2nd Class.*

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*Evidence of Dr. Lloyd, Half-pay Surgeon, 36th Regiment.—  
January 15, 1850.*

I served upwards of 15 years in the West Indies, rather more than four years in Jamaica, and the remainder in the Windward and Leeward Islands.

During this period I have seen remittent fever in two forms, viz., the aggravated, and protracted remittent, the intermittent, and

symptomatic fever, by which I mean a fever arising from irregularity of any kind. I class Yellow Fever under the head of aggravated and protracted remittent. I have seen from 1,300 to 1,500 cases of the protracted form of fever. The general symptoms were preceded by short and repeated rigors, sense of lassitude and weariness of limbs, headache, nausea, sometimes amounting to vomiting; increased temperature of the skin, and in young soldiers fulness of pulse, violent action of the carotid and temporal arteries, flushed countenance, dryness of the mouth and fauces, pain of the loins, occasionally alternating with frontal headache; the latter symptoms amounting to excruciating pain of the loins and forehead. These symptoms appeared in an increased form the following day, attended in some instances with considerable gastric irritation, characterized on the 3rd or 4th day by coffee ground vomiting, preceded in some instances by bilious vomiting, in the protracted form, and in the aggravated form by black vomit; the latter never preceded by bilious vomiting; in the progress of the disease, slight remissions at times only were to be discovered by a very acute observer. These remissions were characterized by the heat being diminished, and the skin becoming slightly moist, and tongue also, which in many instances exhibited the appearance of uncooked beef-steak, accompanied by a slight degree of moisture of fauces. The pulse became more regular and soft, the abatement of symptoms in many instances was very indistinct; but when it did occur, generally took place in the evening, and at irregular periods. I call the abatements that take place in the common continued fever of this country, remissions, on the same principle as above stated; these remissions varied from half an hour to two hours; after these remissions, considerable prostration of strength usually occurred, with increased gastric irritation, sometimes ending in vomiting. When black vomit occurred, the fluid ejected from the stomach resembled tar in a liquid state, imparting an indelible stain to linen or clothes of a greenish black hue; this fluid is ejected violently from the stomach in the form of a segment, to the distance of several yards. The coffee-ground vomit when it occurred, appeared to be ejected in most instances by the action of the stomach alone in large quantities, and not by any jerking of this organ; the stain of this vomit could readily be removed by washing. In some instances there was a slight yellowness of tunicae adnatæ, and towards the back part of the neck in this form of fever becoming more general afterwards, but not invariably; considered by me to be disorganized blood combined with bile. The coffee-ground vomit is a something held in solution by a fluid which, when allowed to stand, settles to the bottom; this sediment consists of a resinous-like matter, tinged with mucus. The black vomit is disorganized fibrin, mixed with bile; the black vomit cases all terminated fatally, in the majority on the 4th day; the coffee-ground cases were some-

times protracted to the 23rd day. During this period gastric irritation was constantly present, but not always attended with coffee-ground vomit, unless the hand was pressed roughly over the epigastrium, when that result invariably ensued. From the 3rd to the 11th day at certain intervals the coffee-ground vomiting occurred; after the 11th day all the symptoms of fever continued, but I do not recollect further vomiting, though nausea often occurred. Remissions in all these cases occurred at some period of the 24 hours during the whole continuance of the disease. In the black vomit cases when examined after death, the stomach towards its inferior fundus was coated with a dark tenacious fluid mixed with mucus, and when carefully removed I could notice dark patches, ecchymosis, and numerous black points; and without the aid of a lens, erosions and minute rupture of the venous coats were to be seen, the arteries appeared to be apparently empty and not much altered in structure; the mucous membrane was thickened with partial and ecchymosed patches of a red colour, varying in size from a fourpenny piece to a sixpence. In these cases the stomach was generally found contracted; sometimes I found a small portion of bile from regurgitation, but no other fluid. In most instances the large intestines were inflated, the mucous membrane not in an abnormal state, the feculent matter sometimes of an ash-colour, greyish, and in no instance assuming the colour of gingerbread; occasionally of a very dark colour. The liver, generally had a diminution of blood in quantity, excepting the portal system, where the blood was generally of a dark colour, its size varying much. In the protracted form of fever which I have often seen at St. Lucia, the liver was soft, disorganized, and of a dark mottled colour, weighing in some instances (drunkards) 12 lbs. Surface often the buff orange of syme, varied with cream-yellow; frequently mottled with these two colours; and could be traced when cut into throughout its substance; its structure friable and greasy. The spleen in the majority of cases was like black-currant jelly, perfectly disorganized, and easily broken down with the fingers. I have occasionally seen cases of coffee-ground vomiting terminate fatally, these were of the protracted form of attack. In these cases, when examined after death, there was an apparent injection of the vessels of the mucous membrane, exhibiting in partial patches redness and ecchymosis; the contents of the stomach sometimes fluid, occasionally empty; generally the fluid was some drink taken into the stomach a short time before death, and sometimes coffee-ground vomit, but never to any great amount. Mucous membrane covered by a tenacious mucus, sometimes of a slight reddish colour. In no instance was there erosion or rupture of blood-vessels. Liver, generally speaking, on its surface was of a light yellowish-brown colour, mottled with bluish green; this colour was generally confined to its external surface. On cutting into it the portal vessels were filled

with dark blood, its structure of a greyish yellow (Syme yellow) varying in intensity. In all the cases that came under my observation of the coffee-ground form, they for the most part terminated in the intermittent character, mostly influenced by locality. I never knew any cases of the aggravated form terminate in this manner, all being fatal. I call the aggravated form of remittent that which exhibits black vomit; the protracted that where the coffee-ground vomit occurs in its course. At Berbice I have seen in the 65th Regiment at least 300 cases of remittent fever, which did not exhibit as a symptom the coffee-ground vomit. The regiment consisted of about 700 men, and the disease was so general that there was not a sufficient number of men (healthy) to mount a sergeant's guard. This occurred in the months of November and December, 1830. Every officer was attacked except myself and one other; I never met a case of coffee-ground vomit here; the regiment suffered much in that fever, the fluid vomited being vitiated bile, and I think about 15 died. In those that died, on dissection patches of ecchymosis were found on the mucous membrane of the stomach, a quantity of thickened mucus, slightly yellowish, adhering to the mucous surface of the stomach; no fluid was found except what the patient had taken into the stomach. Liver enlarged, for the most part friable; extremely unctuous and greasy. Colour between deep reddish-orange to brownish-orange; structure dense, but easily broken down; spleen in most cases small, bluish, and firm. All these cases occurred in barracks. None of the patients in hospital labouring under other diseases were attacked, but I recollect one Orderly suffered who had been greatly fatigued. We had black nurses; none of them were taken ill. At this time there was no fever of this kind in the town; these cases, with scarcely an exception, terminated in intermittent fever, and the intermittents were of a protracted form, few recovered perfectly until removed from the locality. I attribute this fever to marsh miasma. I do not recollect any of those cases to have had a relapse into the remittent form. I consider the cause of the aggravated form of remittent fever to be a highly malarious condition of the atmosphere acting as a deadly poison on the nervous system; and I consider this malarious condition can be at times circumscribed to a limited space, and continue so circumscribed, notwithstanding the prevalence of high winds—I mean in low swampy situations. I never saw a case of a man labouring under any other disease in hospital, attacked with the aggravated form of remittent fever, where patients in the same ward were suffering from it under treatment in hospital; but I separated such cases, ulcers, &c., into other wards, with a view to prevent these cases witnessing distressing scenes occasioned by patients labouring under fever. Some of the Orderlies were attacked by the disease, but rarely in the aggravated form, as very few of them died. In certain localities there was a degree

of panic among the troops, from an impression among them that the disease was infectious. The officers in Jamaica did not entertain this idea; on the contrary, the officers generally of the 50th Regiment were very zealous in assisting and visiting the sick soldiers in the hospital, suffering under the aggravated forms of fever; and nearly all of them were attacked with either the aggravated or protracted form of remittent fever; not one of these officers, so far as I know, had had the fever before. At the time the fever with black vomit prevailed, about four out of ten admissions died, the other six had the coffee-ground vomit. I have never known any of those attacked with the aggravated form, have a second attack of the same form of fever. I have often known relapses where the coffee-ground vomit took place. I have known persons who have recovered from an attack of coffee-ground vomiting, have (six or twelve months afterwards) an attack of the ordinary remittent fever and recover. I have seen the same soldiers attacked four or five times in less than three years, with protracted remittent, particularly men of the Royal Artillery. I consider attacks of the ordinary remittent fever diminish the susceptibility to second attacks.

In September, 1824, when I arrived at Port Royal, Jamaica, there was one company of the 77th and two companies of the 91st regiments. At the time I arrived, there were very few men in hospital either of artillery or line. About the end of November a severe form of fever appeared, the aggravated remittent fever, ending, in some instances, in black vomit; the proportion of deaths was about 1 to 3 treated. The first cases admitted were from the 77th, and I think the first four or five were from two rooms of the 77th barracks, which opened into one another; these cases were scattered over the rooms; they were all single men; after this the cases occurred in different parts of the barracks. At this time the married people were not attacked, they were living outside the barracks in wooden houses, but when the disease extended to the town they suffered, and the troops in barracks became comparatively healthy. The nearest married person's house was about 90 feet from the barracks, some of them extended up into the town; some of the married men were officer's servants at the time, others were performing ordinary duties. About one-third of the strength was attacked. The officers did not suffer at all at this time; no case of fever occurred in the officers' barracks. One officer, however, Lieut.-Colonel Brome, Royal Artillery, died, but he lived in the fort, at a short distance from the officers' barracks, and had been at Up Park Camp on a general court-martial, where the malignant fever was prevailing. The colonel's servants continued healthy. Fever at the town of Port Royal commenced about January; there was no fever at the barracks for six weeks previous to this; the men-of-war were suffering at the time. I was a pall-bearer (one of six) at the funeral of a naval officer, and

in three weeks afterwards I was the only one alive, they having all died of fever. At the time the fever prevailed in the barracks it also prevailed in the shipping (*i. e.* the men-of-war); I think there were four or five men-of-war at anchor, the crews of all the ships seemed to suffer. I never met a case of black vomit at Falmouth, Jamaica, either among the military or civil inhabitants. The fevers, at this locality, were of the protracted form; the majority having the coffee-ground vomit, rarely terminated in intermittent.

In one instance I placed the substance that subsided in the coffee-ground vomiting in a shovel, and placed it over a fire; it gave out a resinous smell and inflamed. At Berbice the officers suffered. When the protracted form prevailed so generally, those inhabiting the lower rooms were first affected, those in the upper rooms were subsequently attacked. At the time the 50th Regiment were suffering from the black-vomit fever, there was a portion of another regiment at the station, the 91st Regiment. Seven officers were present with this detachment, these officers were all in the habit of going into the hospital to see their men labouring under the black-vomit fever. One of these officers, Captain Campbell, and the Assistant-Surgeon, died of the same fever. I do not recollect that any of the other five were attacked with any disease. The officers of the 50th went more into hospital than those of the 91st, from being less apprehensive of the disease, having been longer in the island. I never saw black-vomit fever in Jamaica, excepting in the sickly season, *viz.*, November, December, and January. I have seen an appearance in the mucous coat of the stomach resembling sphacelus, but never could persuade myself it was actually gangrenous, it was of a yellowish dark colour. At Falmouth the mortality was small. I never knew of any case, during my residence in the West Indies, where the Yellow Fever was propagated from one person to another. I do not believe that fevers arising from paludal or terrestrial sources can become contagious under any circumstances. I consider the five pall-bearers above alluded to were exposed to the malarious cause, prevalent in the environs of the Naval Hospital, Port Royal, Jamaica, and thus contracted the disease.

The sources of such poisons at Port Royal are not altogether evident. I never saw a case of intermittent fever at Port Royal, nor at any other station in Jamaica, excepting a few cases at Falmouth. I have never seen any case of black-vomit fever in Ceylon, where I served four and a-half years; in some rare instances, there was coffee-ground vomiting; they did not terminate fatally. Many of the remittent cases in Ceylon terminated in intermittents. About *one in three* treated, however, among the sailors at Trincomalee, admitted into hospital, terminated fatally. The number of admissions was very great out of three men-of-war, the crews were not sufficient to work one of the vessels out of harbour.

I had medical charge of the Naval Dockyard Establishment

there, among the black artificers and labourers (principally from the Malabar coast) as many as 700 were admitted in one month; very few died. The symptoms were of a mild character, a small proportion terminated in intermittent; this fever is commonly known by the name of Trincomalee, or jungle fever of Ceylon.

(Signed) WILLIAM LLOYD, M.D.,  
(Half-Pay) Surgeon, 36th Regiment.

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*Evidence of Surgeon Brown, Grenadier Guards.—January 16, 1850.*

I have seen Yellow Fever only in Gibraltar, in 1828. It commenced about the end of August, and I continued to see cases until the end of October, when I was taken ill with the disease; I saw no cases after that period. I have never seen ordinary remittent fever. I served in Gibraltar as assistant-surgeon of the 43rd, previous to the epidemic between November, 1825, and January, 1827; then in Portugal from January, 1827, till March, 1828, when I returned to Gibraltar with the regiment. During my service in Gibraltar, between November, 1825, and January, 1827, I never saw any case of what is usually called remittent fever. The few cases of fever that occurred in the regiment, I considered common continued fevers. The fevers I saw in Portugal were few and much of the same description. I cannot assign any particular cause for these fevers. The epidemic fever I saw in Gibraltar resembled in its early stage the fevers above described. About the second day irritability of the stomach set in; in the first instance, the fluid vomited was the contents of the stomach; this was followed first by small dark floculi, succeeded by regular black vomit. In the fevers I saw in 1826, when vomiting occurred, the fluid ejected was more or less mixed with bile. One patient, in 1825, died under my charge of the description of fever stated to have come under my observation in 1825 and 1826. I cannot pretend to assign any cause for the occurrence of the epidemic fever in 1828. As far as I could see, there was no difference whatever in the state of the atmosphere, or the town, as regarded filth, drains, &c.

My regiment was quartered at the Casement Barracks from the commencement of the epidemic to about the first week of September, when the regiment was removed to the Neutral Ground, and placed under canvass. The distance of the camp from the gates of the town was about 500 yards. As far as I can recollect, only one case in my regiment occurred in barracks, before we were ordered to encamp. When we were removed, the fever had not reached that part of the town. I continued with the regiment on the Neutral Ground till the middle of October, when I was sent to take medical charge of the 42nd Regiment. During the time

that I was on the Neutral Ground with the regiment a considerable number of cases of the epidemic occurred among the men. No women, children, servants, band, tailors, or shoemakers were allowed to go into the town, unless on duty. I do not recollect any women or children being attacked while I was in the camp, either of the 42nd or 43rd. I do not recollect whether any of the officers' servants were attacked, nor of the band, or tailors, or shoemakers. The cases which occurred were men who were doing the ordinary duties in town; this applies to both regiments. Two officers of the 43rd and two of the 42nd were attacked. All had intercourse with the town. These officers, for some days after being attacked, remained in officers' marquees on the Neutral Ground, attended by their own servants (soldiers); each had one servant. None of these servants that I am aware of were attacked with the disease. During this time there was a moderate current of air right across the isthmus. Each officer was treated in a separate marquee. I ordered the walls of the marquees down during the hot part of the day, to allow a free circulation of air. At all times the circulation of air on the Neutral Ground was greater than in the Casement Barracks. This applies generally to the whole of the town. The circulation of air at Windmill Hill and Europa Flats was equal to that on the Neutral Ground, or rather greater. I was in the Naval Hospital, doing duty there, between the middle of October and the 31st of October, when I was attacked with the disease. I did not see any patients under treatment in the hospital with other diseases attacked with the epidemic. When I joined the 42nd Regiment, all the orderlies and the hospital-sergeant were suffering from the fever, and the hospital-sergeant's wife lying dead of it. I applied for fatigue-men as orderlies, they were relieved every 24 hours, and I knew nothing of the fate of any of them, except one; he volunteered to continue permanently employed, but after three days deserted, was tried by a court-martial, sentenced to confinement, was placed in the Moorish Castle, where he was seized with the fever and died.

A corporal who was sent to assist the sergeant (hospital) of the 42nd, continued to serve during the whole epidemic without suffering from any disease. He lived in the same room which had been occupied by the sergeant and his wife previous to their illness. When I was attacked, I was living in the officers' rooms, in an upper room, under the same roof as the sick. No other officers were living there. My servant, whom I took with me from the Neutral Ground, was attacked previous to me, immediately after coming to the hospital, and died in the hospital. I took a convalescent from the epidemic to attend me when I was ill; he did not suffer from any recurrence of the disease. The Naval Hospital is badly ventilated, particularly the part occupied by the 42nd; they had the back part of the hospital. I could not attribute a single case that I saw to contagion. I do not recollect

any man of either regiment having a second attack. I think, about one-third of the 42nd Regiment had relapses, some of them before they were able to get out of bed, others when walking about. I have known a relapse occur when the patient had been walking about for several days. I could not ascribe these relapses to any cause; they were, I think, generally severe cases previous to relapse, and some of the relapses proved fatal. The rule in Gibraltar was, if any individual could produce a certificate from an acknowledged medical man that he had had a previous attack of the disease in some former epidemic, he was allowed by the laws of the garrison to continue to reside in town, which was not the case with others who did not possess such certificates. I think I have opened the bodies of about 100 cases. The stomach contained usually a considerable quantity of black fluid; it was generally found in every stomach, but when the epidemic was at its greatest height, it was much more frequent than at the beginning or decline. The mucous coat was softened, with an irregular blush of redness on its surface. In almost every case, the small intestines, chiefly the ileum, appeared nearly black, from their contents; the peritoneal coat seemed healthy; the mucous coat as if coated over with black varnish. The mucous coat itself softened, tumid, and of a slightly red blush. Towards the termination of the epidemic, I saw in several cases a greyish substance of the consistence of boiled arrow-root adhering to the mucous coat of the stomach and small intestines. The colour of the liver throughout was much lighter than natural, to wit, buff-orange, varying in intensity; this colour was darker at the commencement and termination of the epidemic; when the disease was most severe, the colour scarcely varied. The structure was softer than usual; when cut into, it seemed to bulge, as if it had been released from something that had confined it; size not altered; spleen appeared not altered; convalescence slow. No case terminated in remittent or intermittent. During the progress of the disease there seemed to be no secretion of bile, at least I saw no evidence of bile having been discharged from the gall-bladder. In the Naval Hospital the heat was excessively great, between 12 and 4 in the day, but not greater than in other years. It was hot under canvass on the Neutral Ground, but there was more or less a breeze blowing, and the walls of the tents were up during the day to allow the breeze passing through. When I performed the post-mortem examinations, Dr. Gillkrest was always present. I know that Dr. Gillkrest performed post-mortem examinations when I was not present, after my removal to the 42nd Regiment; indeed, I know that he inspected all the bodies examined at the Naval Hospital, whether of his own regiment or of other corps. Lieutenant Harris, after his attack, had yellowness of the eyes for some time. I do not consider this effect as a consequence of the disease. I did not treat Lieutenant Harris. Sometimes portions of the dark

matter, stated above as lining the small intestines, were found detached and floating in the tube. I have never seen a case of intermittent fever in Gibraltar, during the time I was quartered there. A number of the convalescents were under the influence of mercury (that medicine having been used in the disease). I have no reason to believe the hospital-sergeant's room had been cleansed and fumigated before the corporal went into it. There were two hospital marquees on the Landport Glacis, established in the beginning of September.

(Signed) G. BROWN,  
Surgeon, Grenadier-Guards.

*Evidence of Dr. Jameson (Staff Surgeon, Second Class).—  
January 17, 1850.*

I arrived in Jamaica 16th March, 1834, and continued to serve there till the 2nd April, 1845, with the exception of about three or four months in 1836, when I was on duty at Honduras. In the mountains of Jamaica I have seen the ordinary synochoid fevers, resembling those of England but milder in form. In the other parts of the island I saw fevers usually designated intermittent, remittent, and the fever commonly called Yellow Fever. I consider the three latter the same fever, only differing in degree. There are no fevers in the lowlands of Jamaica allied to the fevers of this country.

During the whole of 1834 I was exclusively employed in hospital duties, partly at Up Park Camp, and afterwards at Phoenix Park, and was similarly employed for about four years more. The other half of my time in Jamaica I was in charge of the head-quarter staff, in the paymaster-general's office and medical store, and detachment at Kingston barracks. Occasionally during this period, for a short time, I had hospital duties at Up Park Camp. During my service in Jamaica I saw post-mortem examinations of Yellow Fever. In the majority of cases black vomit was found in the stomach; the mucous membrane throughout was more or less softened; in some parts of it red vessels were seen running in different directions, and I think I could sometimes express from these vessels the same fluid as found in the stomach. The softening in some parts amounted to destruction; in others, when taken into the hand, it was easily broken up, and flakes of it were occasionally found in the fluid in the stomach.

The post-mortem examinations were generally performed soon after death; all within thirty hours. The coat I considered disintegrated, but not gangrenous. Generally speaking, the colour of the liver was of the straw-yellow of Syme; in some instances gall-stone yellow of the same author.

In some few cases I have seen it but little altered from its

natural colour. The same colours described extended through the substance of the liver. As a rule, the liver was generally paler than natural; the substance was friable; spleen was softened, colour darker than natural, amounting in some cases to a pulpy mass. In one or two cases the softening of the mucous membrane extended down the duodenum, but, generally speaking, the mucous coat of intestine was not altered. I have seen a dark fluid in the small intestine, analogous to that in the stomach. I have seen, in some few cases, a greenish fluid, thicker than tar, in the intestines. I have seen repeatedly cases of complicated intermittent fever terminate fatally. In these cases there was black vomit found in the stomach. One case, a sergeant of the 56th Regiment, in Phoenix Park, in 1834. I had seen black vomit before; I saw it in this case. This man had been under treatment for some days; I think it probable I was giving quinine and calomel at this time; he may have been ordered sherry, madeira, or brandy; but not port or porter. There was no instance of Yellow Fever at this time in the hospital or neighbourhood. Two of his comrades informed me, that previous to his attack he had been at St. Thomas-in-the-Vale, about 10 miles from Phoenix Park; also free from black vomit fever at the time. My impression is he had been three days ill with intermittent when the disease took on the remittent form.

I define a remission to be a general diminution of all the symptoms present, followed by a recurrence of the severe symptoms. These remissions vary very much, sometimes extending to 12 hours. From what I have seen, they occur generally very early in the morning; say a little before daybreak. I used to visit the hospital before daybreak.

I have never seen simple intermittent terminate fatally, excepting in children. In these cases of children there was no black vomit.

I have seen a few cases of well-marked remittent fever terminate fatally without black vomit. In these cases the mucous membrane of the stomach was softened, and paler than natural. No dark fluid found in the stomach, but a whitish-coloured fluid. The liver seemed natural, in both colour and structure. These cases may have continued eight or nine days before death occurred. The spleen was softened; intestines presented nothing unusual. Vomiting occurred in these cases; fluid vomited consisted of medicines and fluids taken into the stomach, and other ingesta.

I consider that if these cases had lived long enough black vomit would have occurred. The majority of the cases of black vomit fever that proved fatal have had their intellect sufficiently clear to answer questions to the last. I have known some persons die comatose.

In the majority of these cases who died comatose, I found effusion into the base of the brain and ventricles. My observation with reference to this last remark refers to what I observed in the

56th, 8th, 64th, and 82nd Regiments, when I was assisting Dr. Pope (82nd), and Dr. Cardiff (8th). I did not record the post-mortem examinations made by these gentlemen. I called Dr. Pope's attention to these appearances. As regards the brain, I observed nothing more unusual. I have known cases attacked a second time with the severe form of fever called Yellow Fever. I attended Lieutenant Mockles, of the 2nd West India Regiment, in separate attacks; the first in December, 1840; the second in autumn, 1841. In the first attack, this officer had been shooting in Caynaans Lagoons, a short time before his attack. Remissions in his case were distinct, and he was out of danger, I think, the eighth day. He was quartered and treated in Spanish Town.

At the time an epidemic black vomit was prevailing in that locality, and also all over the island. He vomited dark bilious matter. The vomiting in cases which ended in black vomit was similar at first. On one occasion, I believe, Up Park Camp was free from any case with black vomit for three months; I think the September quarter, 1836. I do not recollect during this period any patient in hospital or orderly attacked with remittent fever. When black vomit prevailed as an epidemic fever at different times, I never remember a single case of a hospital orderly or a patient in hospital with any other disease attacked with black vomit fever, except, I think, one or two.

Lientenant Mockle's second attack took place at Kingston, in the autumn of 1841. At Kingston an epidemic black vomit fever was prevailing. His attack was similar to his first, only milder. I ascribe his second attack to exposure to night air. During the malignant epidemic fever, the fatal cases usually terminate on the third day, having had distinct remissions during their progress.

I have never seen black vomit fever occur above 4000 feet.

I consider that when variola is prevailing epidemically the disease is not expected to cease until nearly every individual not previously protected shall have been attacked. I conceive it possible, but not probable, that a person may be susceptible at one time to the influence of a contagious disease, but not be so at another time.

I am inclined to believe that typhus gravior of Cullen, and common continued fever of England, are one and the same disease, but my experience in typhus fever has been very limited. I consider them the same on account of their symptoms being equable, with the exception of the evening exacerbation. After the exacerbation the fever abates, and continues abated until the following evening. I consider that in a marked remittent fever, in the West Indies, after a remission there is a marked rigor, followed by a hot and sweating stage.

In the fevers in this country the paroxysm is indicated by an increase of heat of skin, of dryness of surface, and increased action of pulse. I am inclined to think, according to my views, that all

continued fevers of this country are one and the same fever. I do not wish to give an opinion with reference to the contagious nature of typhus.

I consider all tropical fevers are periodical, and directly arise from the same cause.

This cause is terrestrial miasma, not atmospherical.

The difference of the form of remittent fever depends upon the concentration of this cause and the varying susceptibility of the persons in the locality where it exists.

One positive fact of a case of contagion must outweigh all negative evidence to the contrary.

Let two ships at sea come within speaking distance. Ship A is manned by negroes, unprotected from variola, having had no communication with the land or any other ship. Ship A is perfectly seasoned in all respects, perfectly cleanly, and crew in complete health.

Ship B has left a port a short time previously, also healthy. Ship B sends a boat with a crew and a passenger for ship A, who swings himself on board. The passenger having been exposed to variola on land before leaving, the disease has remained dormant until he arrives in ship A; he there is attacked with the disease, and it spreads through the ship.

This I would consider a positive proof of contagion; I would consider any identical fact with reference to another disease equally an evidence of the operation of contagion.

I do not consider the case of the "Sybille" and the "Black Joke" a parallel case to the imaginary case of variola.

I consider that all periodic fevers of Jamaica must necessarily, from being periodic, originate from the same cause.

I conceive that in the ordinary remittent of Jamaica more cases occur without bilious vomiting than with it.

I consider the portions of the mucous membrane stated as above as destroyed, to have lost their vitality previous to death.

There is not a sufficient elevation of land in Barbados, I consider, to produce intermittent fever.

I consider the attacks suffered by the 50th Regiment at Spanish Town to have been second attacks of periodic fever.

(Signed)

J. ROSS JAMESON, M.D.,

*Staff Surgeon, Second Class.*

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*Evidence of Dr. J. Arthur, Deputy Inspector-General (Half Pay).  
January 21, 1850.*

I saw malignant Yellow Fever first at Gibraltar, in 1810, on board of ship; afterwards in the West Indies. I consider the ordinary remittent of warm climates to be a disease different from the malignant Yellow Fever, as the former has a peculiar aptitude

to terminate in intermittent, which malignant Yellow Fever never does; its peculiarity is its general tendency to terminate in black vomit.

The ordinary or marsh remittent fever occurs in all situations where marshes or uncleared grounds exist. I was more than three years in the West Indies before I saw a case of this description of either marsh or malignant fever.

The first place I saw it was at Morne Fortuné, in St. Lucia, on the top of a high hill, surrounded by much uncleared ground, and the soil stiff clay, preventing the water penetrating, and so retaining it in a favourable position to encourage the decomposition of vegetable matter. There it always prevails, and is frequently very fatal. I was seven or eight months there (between October and June), and it prevailed all the time. The first three years I was at St. Pierre, Martinique, the period during which I saw no cases of remittents, after a hurricane at Fort Royal, the chronic cases of disease at that station were sent to St. Pierre, and amongst them most likely there were some cases of intermittent fever. This station, Fort Royal, I believe to be marshy, and I have understood that remittent fever does prevail there. I have no recollection of ever having seen black vomit ejected before death or found in the stomach after death in the cases of remittent fever, which proved fatal within my own observation. I again saw a great deal of the same fever at Guadaloupe, where I had charge of the general hospital; of the medical cases at Basse-Terre, full of cases of remittent fever, not only from the troops in the immediate neighbourhood, but also amongst those sent from Point-au-Peitre, a station most productive of this disease; sometimes from 60 to 100 cases were in hospital at the same time. I do not think that the deaths amounted to 10 per cent., though many of the cases were admitted into hospital in a very advanced state. I recollect some cases on that occasion had black vomit; and one man in particular was taken very ill in hospital, and on examination I found on the mattress on which he was lying marks of the black vomit, which was black vomit from a former patient who had died shortly before. I could not account for the aggravation of disease of this patient, who was apparently getting well till this time. In consequence I reported the condition of the whole of the bedding in use, which had been taken from the stores of the French, to the Inspector-General of Hospitals, Dr. Ferguson, who ordered a Board to report on it; and the result was that the bedding was ordered to be burnt, which was duly carried into effect.

All the cases of black vomit which occurred at this time were in one and the same ward. Precautions were taken on the occurrence of this fever, *i.e.* prohibiting intercourse between them and the other patients, and enforcing free ventilation. Shortly after this the General Hospital, where this occurred, was broken up, and the sick accommodated in the regimental hospitals, which practice

was continued till the island was given up to the French. On this taking place the English troops were dispersed over the different islands in the command, and this dispersion was followed by a general outbreak of the malignant Yellow Fever.

I was present at Dominica in 1817 and 1818, when malignant Yellow Fever prevailed; also at Tobago, 1818 and 1819; and Barbados, in 1820 and 1821.

In 1820, Yellow Fever was prevalent at Demerara and Berbice, and regulations were instituted at Barbados that no vessels from the stations should be permitted to land any person on the island until duly authorised, after examination by a medical man. One passenger vessel, however, speedily landed her passengers, after a lady (an actress) had died during the passage of Yellow Fever. The governor was anxious to prosecute the captain, but was informed by the attorney-general that there was no law enabling him to do so, and the local regulation was from this time neglected. Yellow Fever began to prevail at Barbados within a short time after this, and continued during the latter end of 1820 and beginning of 1821. During the period of its continuance no cases of the ordinary marsh remittent fever were observed, nor any of intermittent; indeed, I never within my own experience knew a case of the latter disease originate in Barbados. On one occasion, when some severe cases of fever were landed at Barbados from H. M. ships from St. Lucia, I remarked to some of the gentlemen present, "You will see, if any of these recover, they will terminate in *intermittent* fever." This happened as I predicted. The places where I consider remittent fever to particularly prevail are Morue Fortuné, St. Lucia, Somlau Petre, Guadaloupe, and Prince Rupert's, Tobago, in consequence of the Baccolille swamp. The other islands and stations I cannot speak of from personal experience. I recollect Staff Surgeon Panting, when the disease, Yellow Fever, broke out at Tobago, and who was, in addition to his medical duty, in extensive private practice, stating that the disease was so new to him that he thought it his duty to apply to Barbados for assistance. He had himself suffered from Yellow Fever long before at Martinique, soon after his arrival in the West Indies; I think about 1795 or 1796; and that since that period he had seen so little of the disease, and never at Tobago (where he had been many years engaged, as above stated), that he was greatly alarmed at its occurrence. At the time that the malignant Yellow Fever prevailed at Tobago there were some cases of intermittent fever in the hospital in troops who had been sent from Trinidad, but there was no prevalence of cases of the ordinary marsh remittent at the time; the disease Yellow Fever was as prevalent in the town as in the garrison. I do not recollect to have heard what fever was prevalent in the country. Dr. Panting did not sicken of this fever.

I did not suffer from Yellow Fever in the West Indies; I suf-

ferred from it on board ship, in Gibraltar Bay, in 1810. I have known many instances of persons, after having had one attack of Yellow Fever, who were afterwards exposed to it and continued healthy; indeed, I know of no person having had two attacks of the malignant Yellow Fever. I did not see anything that I could call a remission in the fever at Barbados; that is, no distinct abatement of symptoms, followed, sooner or later, by an aggravation of symptoms, such as takes place in the ordinary marsh remittent fever.

The disease which prevailed on board the ship in which I was stationed in Gibraltar Bay, in 1810, presented the characteristics of the malignant Yellow Fever, *i. e.* febrile symptoms, often followed by oozing of blood from corners of mouth, corners of eyes, livid yellowish colour of the skin, and black vomit; the mortality being three in five. Before I went on board, the transports were clean good vessels, and the sick were distributed in these vessels, though they might have been put into one without being crowded. The men of these vessels were clean, good, healthy men, and had been taken good care of; deserters from the French army. These men were in good health previously to touching at Carthage, while malignant fever prevailed there, I think they caught the disease.

The fever broke out in these vessels during their passage from Carthage to Gibraltar.

At Tobago Dr. Cummings' wife, a native of the place, took fever when it prevailed there, and died, and some of the oldest of the population suffered. Several fresh troops arrived in the West Indies between 1816 and 1821. Most of the regiments at Guadeloupe when they suffered had been long in the country; does not think there was one newly-arrived regiment there.

In 1821, when the 21st Regiment was attacked at Barbados, every precaution was taken to prevent intercourse between the sick and well. Before the garrison suffered from the disease every means was taken to prevent intercourse between the garrison and inhabitants of the town and neighbourhood, who were suffering from the disease. The Royal Artillery were the first attacked in garrison; they were encamped close to the barracks, and intercourse with the rest of the troops prevented; and the marked cases were sent to the naval hospital appropriated for the reception of cases of malignant fever. The disease soon ceased with them, but appeared in one company of the 21st Regiment, proceeding from man to man. This company was immediately encamped, and treated as the Artillery were; the disease in them also soon ceased. A second and third company suffered in a similar manner, and were alike treated, with similar results. During this period the remaining companies continued in the barracks in the enjoyment of perfect health.

I think every means was taken to prevent communication between the men in tents and those in barracks. I think about

Three men were placed in one tent. This the barrack in which the 21st were quartered, was the same in which the 2nd Foot suffered so much in 1816, when similar precautions were not taken. I think the rooms in which the men suffered were the lower rooms.

(Signed) JOHN ARTHUR, M.D.,  
Deputy Inspector-General of Hospitals  
(Half Pay).

*Evidence of Surgeon Dr. Millingen, 31st Regiment.—January 22.*

Saw Yellow Fever in the West Indies, *i. e.*, Dominica and Barbados, in 1841-1842, when it prevailed on both occasions epidemically. Was 22 months in the command. The disease commenced in August at Dominica in the 92nd Highlanders, of which I was assistant surgeon. Strength of detachment, 189 men and 7 officers. The health of the troops previous to this was good, excepting slight remittent fevers, which in no instance proved fatal. A ship arrived from Martinique about the first week in August, named the "Funchall." She had come to Dominica in ballast, after having landed a cargo of fish which she had brought from Newfoundland, which she landed at Martinique. She had a crew of 13 men, of whom, on her arrival, I think three or four were suffering from Yellow Fever. I understood the Yellow Fever was prevalent at Martinique when the "Funchall" left that island. The sick of fever of this vessel were landed in the town of Rossena. I think, of seven cases landed, three or four proved fatal; almost immediately after this the disease broke out in the town. The ship was not put in quarantine, and I am not aware that any precautions were taken to prevent such of the crew as were not sick communicating with the population. The disease was pretty general in the town amongst Europeans. The disease was fatal in a great many cases. The troops were not prevented entering the town during this period of sickness. There was a military guard at the time on Government House, situated in the centre of the town.

The fort adjutant, the Commissariat officer, and the Treasury clerks, as well as the Governor and his staff, resided in the town. The Governor had a wife and one child; the Commissariat officer also had a family. The Governor had a batman, a soldier of the 92nd, and one European maid, and his other servants were natives, as well as those of the other officers. There was also Captain Griffiths and his wife, of the 1st West India Regiment, residing in the town. Lieutenant Riley, the fort adjutant, was the first military officer attacked; he died, not having had black vomit, so far as I know and recollect; the body was not opened. This officer, with three or four other persons, had been

on a pic-nic party to a river, some miles off, and had been a good deal exposed, in fishing, &c., for more than one day. Three at least of this party were attacked with fever on their return; Lieutenant Riley, about two days after; the others a few days after him. Of the three, two died and one recovered. I designate a remittent fever one which has, in the course of the day, an abatement of the symptoms, followed by an exacerbation. Lieutenant Riley's fever, with reference to the above definition, was not a well-marked remittent, though I think there was a slight abatement of symptoms, followed by an exacerbation. I think, every morning, whilst the disease lasted, there seemed to be a slight abatement of symptoms. I do not remember his having had any severe vomiting. Dr. Imray, a civil practitioner, attended the other two cases; therefore I cannot speak of them. After Lieutenant Riley's death, I suffered from a slight fever of three or four days' duration, which I ascribed to fatigue from attending on Lieutenant Riley.

About the 16th of August, Major Hort's batman was taken ill of fever, and sent to the military hospital, where he died, I was told, of Yellow Fever. I do not remember any of the sick (about 15 or 16) at that time in hospital being attacked. The hospital was situated on an elevation of 300 or 400 feet, and exposed to a strong draught of wind, which always blew along the Rossena valley.

Mrs. Hort died about two days after the batman. She died of Yellow Fever, but I do not remember if she had black vomit. I never heard of the European servant maid of this family having suffered. A few days after the batman died, some of the men were taken sick. The cases came into hospital from day to day, with sometimes a day or two elapsing without any case. Black vomit then became a common symptom. Lieutenant Gordon was then attacked and died, and he had profuse black vomit and convulsions: the latter (convulsion) was a common symptom in the fatal cases. In a good many cases there was hæmorrhage from the gums and anus. I think there were 205 cases treated, of which about 48 died. The disease continued until the detachment was removed from the island in November; the cases diminished in frequency about the beginning of November.

In one barrack-room the disease seemed to spread on one side of the room, while the other side remained free; it broke out on the windward side, and eventually extended to the other. I do not remember that feature in the other room. The men only occupied two rooms, which were in buildings distinct from each other. All the barracks are ground-floor apartments. When the disease broke out, I had the women and children removed into a separate apartment, distinct from the men. Those of the women who had not already had the disease were nearly all attacked after the removal. Those who had been first attacked in the barrack had been moved into this room, in which the remainder

were afterwards placed, and treated there. The children suffered in a less proportion than the grown up people. I do not remember a child having had black vomit, though Dr. Birrel informed me that a child who had, recovered. At that time I did not consider that the disease was contagious, though now, after reflection, I should not consider myself doing my duty if I did not take precautions which at that time I did not believe necessary.

The stomach after death usually contained a dark-brownish fluid, which was sometimes black, varying in quantity from four ounces to two pints. The mucous membrane generally was softened and pulpy. The vascularity was increased throughout, but never distinct in certain patches. The mucous membrane was easily detached by the handle of a scalpel. On the surface of some of these patches, small dark flocculi were observed, similar to those in the black-vomit fluid, and these could easily be washed off. I did not see any evidence of abrasion, but it appeared as if the blood had oozed from the surface. The mucous membrane was of a pinkish hue, varying in intensity.

The liver did not appear to differ in size, though in structure it was more friable. The convex surface of the liver was of a colour intermediate between gall-stone yellow and gamboge yellow of Syme. This colour pervaded the structure of the organ, as well as the under surface. There was a great deficiency of blood in the organ. The gall-bladder was sometimes empty, seldom distended, but generally about one-third full of dark tarry-looking bile, which bore no resemblance to the fluid in the stomach. I have seen the same flocculi (noticed in the stomach) also in the small intestines, *i. e.*, in the upper portion, and in these adhering to the inner coat.

I am of opinion that this fever was decidedly different from the remittent. The fever throughout its whole course seemed different, and in the Yellow Fever there is a peculiar smell emitted from the body. The ordinary remittent of the West Indies has distinct remission; the vomiting generally bilious. These fevers, in my experience, seldom terminated in intermittent.

Ensign Davis had remittent fever at Dominica, of which he had frequent relapses. The detachment from Dominica was moved to Barbados, and all the men who had escaped the disease, as well as those who had recovered, were present in the garrison of St. Anne's, Barbados, during the prevalence of epidemic Yellow Fever in 1842. When we arrived from Dominica, the 47th Regiment were suffering from fever at Barbados, and we lost one man; the 92nd were under canvass at the Racket Court. At Dominica, I think I recollect some cases of second attacks, where the fever was of the same description as the patient had previously laboured under; others appeared to have been from acts of indiscretion, and the fever seemed different, but cannot speak decidedly on this point.

I was stationed at Ceylon for 20 months; saw no fever there resembling the fever at Dominica as above noticed. The remittent, or jungle fever of Ceylon, partook more of the character of remittent than of Yellow Fever. I saw several officers at Columbo, who came from the interior, suffering from jungle fever. They were very liable to relapses, and frequently required to be sent to England before they could get quit of the disease. Have seen fatal cases of continued fever at Columbo, but never saw black vomit. The Cingalese suffer very much from remittent or jungle fever. I was in medical charge of the civil hospital, where many died, and never saw anything bordering on black vomit.

In Dundee I saw a fever of a typhoid character in the civil infirmary, I think, towards the end of 1843, or beginning of 1844. In the stomach of the fatal cases was found a brown fluid, bearing a resemblance to the black vomit. In this respect, the fever had a resemblance to Yellow Fever, but in its progress it was different. There was another kind of fever prevailing in Dundee at the same time, in which there was at the commencement of the febrile symptom a coloured eruption, about the size of millet-seeds, disappearing on pressure. I did not remark any eruption in the cases where the dark fluid was found in the stomach.

In the fever which occurred at Dominica, the convalescence was slow, but more slow when bleeding was employed. I think I must have opened about 40 bodies in the West Indies, and although in some black vomit had not been ejected during life, it was invariably found in the stomach.

My experience leads me to believe that the smell emitted from the bodies of Yellow Fever patients is different to that which I perceived in cases of remittent fever in the West Indies or Ceylon, or in any other disease, indeed.

*Examination continued.—January 23.*

In addition to the persons mentioned as residing in the town of Rossena at the time of the outbreak of fever there, there was also Lieutenant Monro and his wife. This officer was preparing to embark for Europe at the time the fever commenced, but before going on board the vessel he suffered from a slight indisposition, which I attributed to exposure to the sun and exertion in making preparation. He was confined to bed for 24 hours, and in 48 hours he was enabled to continue his preparations, which lasted for four or five days. He embarked in the "Ealing Grove," and on the day he embarked I went on board this vessel, where I found a sailor, in my opinion, labouring under Yellow Fever, lying on the deck. Notwithstanding this, the vessel put to sea in a few days. Besides Lieutenant Monro and his wife, there were passengers, Lieutenant Lacey and a boy named Blanc, about nine years old, going home for education. Letters were subsequently

received, stating that Lieutenant Monro died six days after embarkation of Yellow Fever, also the young boy, about the same time, and that Lieutenant Lacey had suffered from the fever in a severe form. Captain Faulkner, the master of the vessel, was an old West India trader, who, I think, was likely to know Yellow Fever. I heard that the sailor above referred to died. A setter dog, belonging to Lieutenant Hales of the 92nd, who came from St. Lucia to Dominica, became ill. I saw the dog: the conjunctivæ, gums, and inside of the mouth were strongly coloured yellow, and when the dog died, I found in the stomach some fluid (two or three ounces), exactly resembling the black vomit. This officer arrived during the time Yellow Fever was prevailing at Dominica.

(Signed) H. MILLINGEN,  
Surgeon, 31st Regiment.

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*Evidence of Staff Assistant-Surgeon Dr. Webb.—January 23, 1850.*

I served in the West Indies from November 1845 to July 1849; during that time I was stationed at St. Lucia and Barbados. I saw Yellow Fever at St. Lucia and Barbados. At the former island I saw three or four cases of fever, which I now consider to have been Yellow Fever, for the skin was yellow, great gastric irritability, and the peculiar appearance of countenance characteristic of Yellow Fever. The fluid vomited was first of a greenish tinge, and afterwards became of a chocolate colour. The vomiting lasted for two days, more or less. None of these cases died. The disease lasted five or six days, *i. e.*, till I considered them out of danger. I returned some of these cases as febris remittens.

I define remittent fever to be, a fever in which abatements of symptoms, followed by exacerbations, occur. Such phenomena took place in the fevers above referred to. With the exception of these cases, the troops were not at that time (I think, June and July) suffering from any other fever, nor did they during the time I continued on the island, which was till February following, with the exception of some slight cases arising from drink.

In the middle of December 1847, I saw Yellow Fever at Barbados, and continued to see cases until the early part of 1849, when the disease ceased there. I observed no remissions in the cases of Yellow Fever which I saw in Barbados.

I saw cases of Yellow Fever during the prevalence of the disease at Barbados, which were not so severe as those I saw at St. Lucia; and in these cases the fever was continued. Notwithstanding, I consider that these cases, and the cases I saw at St. Lucia, were one and the same disease. I think I was present at

more than 100 post-mortem examinations of subjects who died of Yellow Fever at Barbados. The stomach was distended with gas, and contained more or less black vomit. I do not recollect having been present at the opening of any body when black vomit was not found. The mucous membrane was, generally speaking, softened, and reddened in patches, chiefly at the cardiac extremity. The patches were from ecchymosis, and were of a colour intermediate between purplish red and lake red of Syme. Patches varied from a fourpenny piece to size of a half-crown. The surface of the patches was smooth, and sometimes flocculi, like those found in the stomach, were observed. The liver was soft and friable, and the vessels were congested with blood. The surface-colour nearly the wine-yellow of syme, waxy yellow. I only saw one exception to this—Dr. Irvin, assistant-surgeon, 72nd Regiment. His liver was smaller than livers usually are, and excessively congested, and of a dark colour. The colour of the substance of the liver was in all instances the same as that described to be that of the surface. Gall-bladder was sometimes full, and sometimes all but empty; generally of a dark colour, and inspissated. I observed black vomit generally, both in the large and small intestines; it was floating in the tube. Towards the stomach, the mucous membrane was of a dark colour. Did not observe any organic disease in the intestines. The spleen was a little softer than natural, but nothing remarkable in point of size. I have never, within my experience, known a person to have had a second attack of Yellow Fever. I saw no case of relapse. I consider Yellow Fever, and the ordinary remittent fever of the West Indies, to be specifically different. In remittent fever, rigors usually usher in the disease. In Yellow Fever, rigors do not generally occur; the patient only experiencing a slight chilliness. The appearance of the countenance is different in the two diseases, but the exact difference not to be conveyed by language; but I think I could always discover the one disease from the other by this peculiarity: when the disease, *i. e.*, Yellow Fever, is established, there is a greater degree of general excitement and alarm, and more intensity of symptoms, than in the remittent fever. In Yellow Fever, the eyes present the appearance of those of drunkards, which they do not in the remittent. I saw some cases of remittent fever amongst the black troops at Barbados. I consider the Yellow Fever at Barbados to have been produced by some specific poison, assisted by some atmospherical causes. I think the poison to be an emanation from the ground, and absorbed into the blood through the lungs. I do not think the disease is capable of propagating itself by contagion. Some cases occurred at Barbados which had the appearance of having arisen from contagion; viz., the Ross family, residing in a house in the New River road, on the margin of the river, about a mile and a-half from the barrack, opposite to which, when the tide is low, there is a quan-

tity of slime and mud, and stunted vegetables. Round the house there was no accumulation of filth (so far as I know) within the circle of 100 yards. There are other houses occupied by respectable persons; none of these persons suffered at the time the Ross's did. The daughter of Mr. Ross was first seized, about November 1848; the mother next; then the father, and then the son, who, *i. e.*, the son, had been residing in the house from the time that the family were attacked. The mother was an aged woman. One other extraordinary circumstance is the case of the hospital bath-man and hospital servants, and sick of other diseases, being attacked; but I think I can account for their suffering from the operation of local causes. The bath-man's wife was attacked, and then the husband, who attended her. On the other hand, when Lieutenant Norrie, R.A., suffered from the disease in March, there were several officers lately from England living in the same house, and they attended him; the room was well ventilated. None of these officers suffered an attack for three weeks afterwards, when Lieutenant Romes was attacked. Two or three artillerymen were in attendance on Lieutenant Norrie. This officer vomited blood before the black vomit appeared.

I have known a case of typhus fever occur in a clergyman's family in England, without the disease extending to any other member of the family, eight in number. Promises to furnish a written record of some more cases where the disease did not spread. Convalescence slow, the digestive organs being weakened. I consider that the commonest diseases often pass through a family in succession, without any suspicion of contagion.

Dr. Webb was, during this period, in charge of the apothecary's store, and only occasionally called upon to assist in the medical duties of the garrison, except on one occasion, when, for about 10 days or a fortnight, he had medical charge of the Royal Artillery during the illness of Senior Surgeon Whitelow; the artillerymen had not at this time begun to suffer. He was frequently in the regimental hospital of the garrison.

(Signed) H. MARCH WEBB, M.B.,  
*Staff Assistant-Surgeon.*

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*Evidence of Staff Surgeon (Second Class) Blakeney.—  
January 28, 1850.*

Served in the West Indies from February, 1838, until May, 1840. Saw Yellow Fever at Demerara and Barbados.

I consider Yellow Fever a different disease from the ordinary remittent fever of the West Indies. I mean by remittent fever, one marked by periodical remission of symptoms followed by exacerbations. At Demerara I saw, I think, about 50 cases of Yellow Fever in 1839; the fever was of a continued form, and

black vomit was a common symptom. I performed all the post mortem examinations. The mucous membrane of the stomach was generally thickened and softened with numerous echymosed patches and points of extravasation. In some cases there was general increase of vascularity, saw no detached portions of the inner coat. The stomach invariably contained the black vomit in more or less quantity. The liver usually enlarged and much gorged with blood. Structure friable and easily broken down. The colour on the convex surface was generally straw-yellow, in some cases ash-grey. I do not recollect the colour of the substance of the liver; gall-bladder sometimes distended with viscid bile, and in other cases it contained but little. I do not recollect the appearance of the contents of the intestines; spleen was usually enlarged, structure disorganised, much softened, and readily broken down; I can assign no cause for the disease. I recollect a quantity of bush to windward of the hospital was cut down, and a quantity of vegetable matter exposed; this occurred in other years when no Yellow Fever appeared.

The patients with Yellow Fever were not isolated from the other patients. I do not recollect any case of Yellow Fever originating in the hospital. The disease was general in the regiment, and not confined to any particular locality. We never thought about contagion, we were very much occupied, nor did either men or officers think of contagion so far as I know.

I do not recollect any person having suffered a second attack of Yellow Fever. I do not recollect any cases of relapse, no officers on this occasion suffered from the disease. I do not recollect any case of Yellow Fever terminating in either remittent or intermittent fever; a number of intermittent fever cases were treated, some of which merged into the remittent form. I do not recollect any cases of remittent fever having passed into Yellow Fever; the first cases occurred amongst the military. The 67th Regiment had been stationed in the different islands in the command since March, 1833; hence had been between five and six years before the outbreak of the disease. There was a number of vessels in the river at the time.

I do not recollect that any case of Yellow Fever existed in the town when the military were attacked. Before it ceased among the military the crews of the ships began to suffer, and afterwards it extended to the civil population. I believe it was very fatal to the shipping, and also in the town.

I was informed by an old resident that previous to this invasion of Yellow Fever there had been no invasion of the disease for 15 years.

Saw Yellow Fever in 1840 in Barbados, the sick having been disembarked from one of Her Majesty's vessels, "Vestal," who were treated in the 67th hospital, of which I had charge. This vessel was stated to have a quantity of unbarked wood on board, for what purpose does not know.

The sailors stated that a few days after they sailed from Trinidad the stench was horrible from this, and that many of them sickened. I think there were about 40 men sent sick.

Hæmorrhage from nose and gums not common at Demerara.

The 76th Regiment, which relieved the 67th at Demerara, suffered very severely from Yellow Fever.

From my present impressions I should not separate Yellow Fever cases from the rest of the patients.

(Signed) G. H. BLAKENEY,  
*Staff Surgeon, Second Class.*

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*Evidence of Sir William Pym, Inspector-General (Half-pay),  
Superintendent-General of Quarantine.—January 29, 1850.*

I consider Yellow Fever and the ordinary bilious remittent fever of hot climates to be specifically different diseases. Remittent fever exists in many parts of the world; in Syria, in the Levant, in India, on the coast of Africa, and in different parts of the West Indies, and its cause is well known, viz., exhalations from marshy or uncultivated grounds, and in those only. In many of these places it is a disease of season; in others, as the coast of Africa and perhaps certain localities in India, it is constantly prevalent. These remittent fevers exist in different degrees of severity, such as at Walcheren, Minorca, the coast of Africa, and the East Indies. The disease at the two first stations I consider to be a much milder form than the highly concentrated form of the jungle fever of India and the remittent, as it occasionally occurs on the coast of Africa. I consider that the name bilious remittent fever\* clearly indicates the nature of the disease, being attended with regular remissions, followed by exacerbations with increased action of the liver. I consider this fever to be essentially different, from persons who have had one attack of remittent fever being rendered highly susceptible to a second attack when exposed to malarious influence; persons also who have had an attack of remittent fever are very liable to be attacked with intermittent fever, followed by visceral disease, particularly of the liver and spleen.

I have seen a great deal of the remittent fever, particularly at Port Royal, Martinique, and in Sicily, and three cases in Gibraltar,\* who were in gentlemen who had been exposed in a damp situation in Spain. On their return they were attacked in succession with a severe form of remittent fever, characterized by regular remissions, followed by exacerbations and sickness at stomach, with bilious vomitings. I have not seen a case of intermittent fever which proved fatal examined after death, but I have seen cases during life where the liver was manifestly enlarged. At the period I speak of post mortem examinations were not generally made.

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\* See Return, pp. 94, 95

The Bulam fever differs first in being more rapid in its progress has no remission followed by exacerbation, although there may be occasionally an apparent alleviation of symptoms. The Bulam fever, unlike the remittent fever, which exists only in unhealthy situations, prevails in all situations as well as at all seasons of the year. One attack makes the human frame proof against a second, in the same degree as small-pox.

Patients having suffered from this disease are not liable afterwards to suffer from intermittent fever as the termination of the disease, nor do they suffer from visceral diseases as the consequence of the attack of Bulam fever. Patients are very frequently attacked with a very mild form of Bulam fever, this mild form gives the constitution the same security from a second attack as a severe one.

This mild form is by many people put down as a case of the highly aggravated form of the remittent. A marked difference also in the two diseases is the increased action of the liver in the former (remittent), and the apparent total want of secretion of bile in the other (Bulam), throughout the disease; that is, that in the Bulam fever in a severe form the liver soon loses the power of secretion, as well as do the kidneys.

The convalescence of those who recover is rapid. To constitute the genuine remissions of which I have spoken, I consider it necessary that there should be a great diminution of the febrile symptoms, the exacerbation following ushered in by chills, sometimes rigors, with a return of the sickness or vomiting.

While I was in the West Indies the Bulam fever prevailed at all seasons of the year.

After the taking of Guadaloupe I was sent off to Martinique to join the 70th Regiment, in consequence of the surgeon and 111 men having died in the month of April, 1794, in that island, from which time it continued to exist at all periods for two years without intermission. All new arrivals from England being soon attacked.

In the Bulam fever black vomit was very general, almost in every case, and in the few cases which were examined it was found in the stomach. The patient in the Bulam has sometimes been supposed to be convalescent from the total cessation of all complaint, fancying himself quite well, but on making even a slight exertion nearly fainted, after which black vomiting came on immediately, which continued from 1 to 14 hours, when the patient died.

I was at the capture of Martinique in 1794, and when I left it with the expedition for St. Lucia and Guadaloupe, the troops and population at Martinique were perfectly healthy. After being at Guadaloupe two or three days an express came to the Commander-in-Chief, reporting the outbreak of fever in the 70th Regiment at Port Royal, Martinique. The disease showed itself first in a casemate-barrack in Fort Edward, its origin could not be

accounted for; about the sixth or seventh day after I joined the regiment, as I have already stated, I found several of the officers and men ill; I was attacked myself the sixth or seventh day after my arrival, having previously recommended the removal of the regiment to Point Negri.

With reference to the allusion to Carpitole in Sir W. Pym's work, page 8, the author is not prepared at present to state how long the regiment remained at Point Negri before they were removed.

With reference to Gibraltar, I believe that Louis mentions that from its being believed that children suffered less than adults from Yellow Fever, parents were in the habit of exposing their children for the purpose of contracting the disease, so that they may escape it when they became adult, and liable to suffer more.

I have seen a great number of vessels arrive in the West Indies while I was there in the years 1794 and 1795, which lost so many of their crew from Yellow Fever that hands did not remain to man the vessels.

I consider a contagious virus cannot originate *de novo*, that when any contagious disease breaks out in any locality there must have been there a virus previously generated, but previously inactive.

I have heard that Bulam fever broke out in Chichlana, or Medina Sidonia, in Spain, at a time when it was not known to exist in any other part of the country, and its outbreak could not be accounted for. When the disease prevailed at Cadiz on another occasion, I heard that a *cordon sanitaire* was established which prevented the introduction of the disease into this village.

I do not consider that a great number of cases of disease occurring at the same time is a proof of contagion. I believe that the ordinary remittent can never under any circumstances become contagious.

In 1810 the measure I recommended was to place a sentry on every infected house, till the sick were removed to the neutral ground, and a cordon of troops round the infected locality; and I believe every person infected with the disease was included, except, I think, Captain Boyd who was at a distance up a hill (also drier), and a priest who was removed to the neutral ground. I knew three officers, General Binkeley, Colonel Garconne, and Colonel Otway, of the Artillery, who having had Yellow Fever previously, escaped an attack when lately exposed at Barbados.

(Signed) W. PYM.

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*Evidence of Dr. Gillkrest, Inspector-General (Half-Pay).—  
February 5, 1850.*

Exemption from second attacks of Yellow Fever in Gibraltar, in 1828, was very great; this exemption is a popular tradition long established throughout Spain.

Submitted a list of 23 cases of second attacks, a copy of which he promises to furnish to the Board. I think it likely that some of the cases were produced to the Board assembled in Gibraltar in 1829, to inquire into the liability or non-liability to second attacks of Yellow Fever.

The belief of persons not being susceptible to second attacks was acted upon in Gibraltar in 1828, with regard to the selection of persons being exposed to the disease. Civilians on this account were employed as hospital attendants in military hospitals, I do not recollect how many soldiers, who were employed as orderlies in hospital, were attacked with the disease. Mentions 69 fatigue men of 43rd Regiment who were employed as orderlies in attendance on the sick.\* List to be given to-morrow, showing the proportion attacked. I do not know that any of the civilians who acted as sick orderlies were attacked.

Some old families in Gibraltar held the opinion that children being born in Gibraltar enjoyed as great an immunity from the disease as those who had undergone an attack. The same belief is held in Spain by many with regard to persons born or long resident in the West Indies; that is, that they were equally protected as if they had had an attack of the disease. Berthe and others mention this.

I believe it was the opinion of some that children had the disease generally in a very mild form, and in consequence it was wished that their children should contract the disease at an early age.

I consider that typhus fever is a disease that gives immunity from second attacks of that disease, but from my long experience I do not consider that disease contagious; and I found this upon the fact of my having, in the course of my services as regimental surgeon, seen the disease in hospital almost every year, and in no case whatever have I witnessed its transmission from the person of a soldier labouring under the disease to that of others. Not even after the Corunna retreat, when our hospital in the 43rd Regiment may be said to have been crowded with typhus in its very worst form (in the hospitals at Colchester), on which occasion the integuments of the lower extremities absolutely sloughed away, as was witnessed by Sir J. Webb, then senior medical officer there. The disease altogether remained confined to the individuals who had passed through the severity of that severe campaign.

After close attention, I believe the epidemic Yellow Fever of Gibraltar in 1828 was of the same family as the severe remittent fever of tropical climates; both arising from malaria in its extended sense, and not confined to the miasms from marshes, but may be produced by a deficient element in the atmosphere, or an element superadded. We know that in its production epidemic-

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\* This list will show that the proportion attacked was something less than men of the regiment not employed as orderlies.

ally, heat though not to an extreme degree is essential, say between 60 and 75 or 80; sporadic cases may occur at a lower range.

Locality influences its appearance very much, as lately exemplified in Barbados and many other places.

The great majority of epidemics throughout Spain as well as Gibraltar have been in the last third of the year, that is, from about the middle of August to the last day of the year, as admitted by the great authority Arejula, and many others.

The arrival of fresh troops in the West Indies always furnishes fresh materials for the prevalence of Yellow Fever epidemics.

I consider that an attack of intermittent and remittent fevers of tropical climates does not afford an immunity from a second attack, nor do I think that one attack of Yellow Fever furnishes an absolute security from another.

I cannot speak positively as to whether one attack of remittent fever increases the susceptibility to a second attack.

I had some men in the 43rd Regiment who suffered repeatedly from remittent fever and agues. I cannot speak with reference to relapses in remittent and intermittent fever; I do not recollect having seen any cases of remittent terminate in intermittent, but I may have done so. I do not recollect any cases of Yellow Fever having terminated in intermittent fever. I am satisfied, however, from my researches as to occurrences in various parts of the world that it did occur, especially on a certain occasion, I believe, at Malaga, as to be found in the work of Arejula; and this was noticed also by Dr. Rush, of Philadelphia, in the great epidemic there of 1793, where it was very common.

*Examination of Dr. Gillkrest continued, February 6, 1850.*

I am able, from my own experience, to state that relapses in the Yellow Fever of Gibraltar in 1828 were common. Hands in a list of cases which occurred in his own corps (43rd) as well as in others.

I do not recollect ever having seen a man who had been discharged from the 43rd hospital cured from an attack of Yellow Fever, return from the barracks with a relapse of the disease.

All my experience in the West Indies in two severe epidemics of Yellow Fever, one at Fort Edward, Martinique, 1801, in the 1st battalion, 68th regiment, lately arrived; the other in the 2nd battalion, 68th, at Dominique (Morne. Bruce) in the year 1801; also at Gibraltar in 1828; and the result of my deep reflection on the subject, as well as the consideration of its history in different parts of the world; goes fully to the strongest conviction upon my mind of Yellow Fever not being contagious under any circumstances whatever. That even crowding of many patients together, though perhaps liable to produce other com-

plaints, will not produce Yellow Fever. In this respect it resembles ague which cannot be produced by crowding.

In support of this, I refer to documents to be furnished, particularly to those which allude to the 69 fatigue men, and to servants of the hospital. None of the latter were attacked for several weeks after the disease had been treated in hospital. The fatigue-men referred to were sent to the number from 1 to 3 daily to assist the regular servants (orderlies) in the hospital duties.

Promises a document with reference to washerwomen, showing that persons washing the clothes of those attacked with the disease were not attacked in consequence. Promises a document, showing members of families were not attacked though in communication with other members suffering from the disease.

Promises a document, showing that patients in hospital with other diseases were not attacked with Yellow Fever until the inhabitants in the immediate neighbourhood were affected with the disease. Promises a document with reference to the immunity of medical men from the disease until a late period of the epidemic. Promises a document, showing that men on guard were exposed to local causes. Promises a document, showing that families left the town for the neutral ground with their bedding, &c. at a time when some members of them were affected with the disease, but the disease did not spread. See PALLONI on epidemic of Leghorn in 1804.

*Dr. Gillkrest's Evidence continued.—February 7, 1850.*

Mentions in favour of the local origin of the disease in Gibraltar in 1828, the following case:—Mrs. Farquhar, a respectable lady and very old inhabitant of Gibraltar, had a young lady, a niece of her's, living with her at a very comfortable cottage in the South, a part of the site of which was actually cut out of the rock. This niece, I believe, had arrived from England but a few years before. Mrs. Farquhar repeatedly told me she had had the disease herself in a very severe form in 1804, in proof of which she told me she had a glandular swelling in the groin which proceeded to suppuration. Her husband also had a disease in the same year (1804). She seemed to think that this circumstance rendered her insusceptible in 1828; she thought it right, however, on account of her niece residing with her, to cut off all communication from persons beyond her gates. I was among this number of persons excluded. There was some space between the house and road, so that no person in the latter could come close to the house. The niece, however, was attacked in the severest form that could be imagined, and died.

States that two families in the same house with Trotoabas, each with double the number of members in their families (eight in each) without seclusion escaped the disease. Mentions that

Martinez was, during the time the disease was prevailing, going about the town pursuing his occupation, his chief precaution being, not going out at night.

*Examination of Dr. Gillkrest continued.—  
Friday, February 11, 1850.*

I do not think any orders were given to men of the 43rd regiment to change their clothes when they returned into their own tents from the town. I do not recollect whether the pay serjeants of the 43rd regiment suffered more than other men of the corps.

Q. How is it that if several elements coexist together sufficient to be a cause of a sporadic case of Yellow Fever, that the disease thus produced does not spread?

A. I cannot otherwise account for this, than by it not being contagious, and that a very particular susceptibility exists in the sporadic case attacked. I may mention that the occurrence of scattered or sporadic cases is according to several good authorities; and is of itself fatal to the doctrine of contagion.

At the time the 68th regiment suffered from Yellow Fever at Martinique in 1801, I believe the disease did not prevail among the civil inhabitants. I served in the West Indies between 25th October, 1800, and 12th February, 1803, during which time I saw Yellow Fever epidemically at Martinique and Dominica. Again, I served at Gibraltar between 17th March, 1828, and November, 1829, during which period I saw Yellow Fever which prevailed epidemically in that garrison between August, 1828 and January, 1829.

I think electricity may have something to do with the production of Yellow Fever.

I will furnish on Wednesday next the 14th instant, a copy of a report on the subject of Yellow Fever drawn up by me, and which contains the various documents alluded to in my oral examination, as well as others, likely to be useful to the Board in their enquiries.

(Signed) J. GILLKREST, M.D.  
*Inspector-General of Hospitals.*

*Dr. Gillkrest re-examined.—February 12, 1850.*

I saw several cases of sporadic Yellow Fever in Gibraltar, during my service in that garrison. I recollect one terminating fatally in the upper part of the town. I do not recollect if there was black vomit in that case.

I saw black vomit in one case; the patient was a sailor from a man-of-war in the bay, and was treated in the Naval Hospital. He died. The military medical records of Gibraltar for some years past give many other cases, but within the last 25 years there have been very few instances indeed compared to previous years. At the time the sailor alluded to was attacked, other

sailors were received into my hospital, from another ship of war treated there by my orders. They had yellow skins and other severe symptoms, but no black vomit. These cases had remissions, some perhaps had not. I do not recollect.

The ship from which the fatal case was received came direct from England. The disease did not spread to any persons in the hospital nor to the medical officers.

(Signed)

J. GILLKREST.

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*Evidence of Hugh Fraser (H.P.) 60th Rifles.—  
February 12, 1850.*

I saw Yellow Fever in Gibraltar in 1828. Never saw the disease on any other occasion. I have seen remittent fever in the Ionian Islands and Canada. I consider remittent fever and Yellow Fever to belong to the same family of diseases, viz., to those occasioned by terrestrial malaria, in the extended meaning of the term; but I consider the cause of remittent fever must be somewhat different in power to that which gives rise to the more continued form of the disease known under the name of Yellow Fever.

I am not prepared to offer any opinion whatever as to what this cause is. In the remittent fevers which I observed in Santa Maura the remissions were very distinct. I think I saw about 12 cases, and these only after the disease had existed for three days, they having been sent from Santa Maura to Zante for treatment. The remissions I state as having been present were characterized by an abatement of the febrile symptoms, lasting sometimes nine hours, and followed by marked exacerbation of fever, not always with rigors, but generally chilliness. They occurred sometimes daily, at others every second day. Of these twelve cases, four or five I think, proved fatal, between the sixth and tenth day. In these cases, in no instance did black vomit appear before death; the matter ejected from the stomach was chiefly composed of ingesta.

Generally speaking, the stomach in these cases was irritable, with great tendency to vomit. I cannot recollect that I ever saw any bilious fluid vomited. The feculent discharge from the bowels, which were generally relaxed, was dark coloured. The dark colour appeared to arise from blood or diseased bile. The vessels of the stomach of the fatal cases were turgid. The mucous coat was pulpy, softened, and more vascular than in a healthy condition. This vascularity occurred in patches, especially about the two orifices. No abrasions or destruction of parts were observed. Stomach was generally empty. I do not recollect that any fluid was contained in the stomach. The liver was generally engorged. When cut it was softer than natural, and blood was generally discharged in considerable quantity. The colour of the viscus was generally dark. The spleen generally enlarged, substance softened. I do not recollect that any of the twelve cases

terminated in intermittent, but I have had cases from Santa Maura labouring under intermittent, which had been preceded by remittent.

Remittent and intermittent fever are common diseases in the Ionian Islands.

I should not say, as far as my experience goes, that these diseases are fatal diseases in these islands.

In 1831 a severe form of remittent fever had prevailed near the falls of Niagara,\* but had ceased there before I arrived. Anxious to make myself acquainted with the disease, and hearing that it was still prevailing along the banks of the river at Buffalo in the United States, about twelve miles from Niagara, I went there; and from what I saw and heard from private practitioners there, I was inclined to consider the disease as identical with the fever that prevailed in Gibraltar in 1828. I saw there cases of decided remittent fever, and others which showed no remissions, only a slight abatement of the febrile symptoms, followed by an exacerbation of fever. I considered some of these cases as having presented the same characters as the disease in Gibraltar did, though it is impossible to describe what they were. I saw no instance of black vomit, but was told that several had occurred. The private practitioners distinctly told me they considered the disease then at Buffalo was the Yellow Fever of the American continent. This occurred in the latter end of June and July. The disease broke out immediately after the breaking up of the frost. A number of people residing between Niagara and Fort Erie, on the English side, were attacked. From Fort Erie it extended itself to Buffalo.

I remained at Niagara for three or four years, and during that period I do not recollect having had any soldiers in hospital suffering from intermittent or remittent fever. I did not witness either disease in any other part of Canada.

I consider the remittent fevers at Buffalo to have all had their origin from one and the same cause, that cause differing only in power. I do not consider that a person having one attack of remittent fever derives therefore an immunity from a second attack of remittent, but rather that it predisposes to an attack of intermittent fever. I consider that one attack of Yellow Fever gives a very great protection to the individual attacked, from a second attack of Yellow Fever, but, perhaps, not more so than in typhus fever or cholera. At present I do not consider typhus fever to be a contagious disease. I consider no diseases to be contagious, except the Exanthematous diseases, and they, not to the extent that is generally believed. I knew several cases of persons suffering from Yellow Fever in Gibraltar in 1828 who had suffered from the disease in that garrison in previous years. The proof of this was the testimony of the persons attacked. The popu-

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\* Lundy's Lane.

lation generally was under the impression that one attack saved them from a second. I served in Gibraltar between 1823 and 1829. I never saw, during this period, any cases of remittent fever, so decidedly of a remittent character as those that occurred at Santa Maura and Buffalo. I saw cases of intermittent fever, but I cannot say they originated on the rock. Along the coast of Spain I occasionally saw cases of remittent and intermittent fever, particularly at the first river from Gibraltar on the eastern coast. The country in this direction was marshy. I never saw anything in the epidemic of Gibraltar in 1828 that led me to believe the disease was contagious, but the contrary; during the prevalence of the disease my wife, wife's sister, and female servant resided on the neutral ground, where I slept occasionally and visited frequently. None of them were attacked with the disease. I was taken ill towards the end of the epidemic; my wife, against my wish, got into town and attended upon me; in two or three days she was seized with the disease in a severe form, still was convalescent in the course of a week. Mentions another case, Lieut. Werge, 12th regiment, was quartered in the neutral ground, was attacked there, was treated in my house in the Civil Hospital, became convalescent, returned contrary to my wish to the neutral ground; went and drank tea with Mrs. Fraser; had a relapse while with her, vomited black matter in the tent, and returned to the Civil Hospital, where he died. This was more than two weeks before Mrs. Fraser was attacked.

The liver in fatal cases of Yellow Fever was of a fawn colour. (Promises a document, giving particulars of the morbid appearances of the disease.) Several cases of relapse occurred even when individuals had been two or three weeks out of hospital.

Convalescence generally, was rapid and perfect; leaving no organic diseases behind, as far as I know.

Maria Piscadina, my servant, was attacked with fever on 3rd August, before the disease was declared as established. She had irritability of stomach and yellow skin, but I did not consider it a serious case, otherwise I should have sent her to hospital. I did not require to get another servant to do her work. She recovered, and some time after had a second attack rather more severely. Both attacks occurred in the month of August.

The disease generally ran its course rapidly; if the patient got over the third day into the fourth he generally recovered, and was convalescent about the eighth. No precautions, I believe were taken by the medical men in Buffalo to prevent communication between the healthy and sick on the presumption that it was an imported disease.

I consider, sporadic cases occurred in Gibraltar between 1823 and the breaking out of the epidemic in 1828; some of which were fatal.

(Signed) HUGH FRASER,  
*Half-Pay Surgeon 60th Rifles.*

REPORT of Dr. BURRELL to the LORDS of the COUNCIL, on the reasons for the opinions delivered by him as Member of a Board of Inquiry, held at the Office of the Army Medical Department (1849-50), on the subject of Yellow Fever.\*

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AS A MEMBER of a Board, assembled by order of his Grace the Commander-in-Chief to investigate and give an opinion on the following points, viz. :—

1st. Does the Yellow, or Bulam Fever, differ from the Marsh, or Remittent Fever, of warm climates, or is it the same fever in a more aggravated form?

2nd. Does an attack of Yellow or Bulam Fever give, like small-pox, an immunity from a second attack, except in very rare instances?

3rd. Is Yellow or Bulam Fever a contagious disease?

4th. Is it capable of being imported?

I consider that the weight of testimony, both oral and documentary, brought forward in the course of this inquiry, as well as a careful examination of the whole subject, will fully warrant me in submitting the following observations.

I BELIEVE Yellow Fever to be the most concentrated grade of fever in all countries capable of generating it,—to be closely allied in its nature and causes to the ordinary fevers, more especially in the Antilles, whether the type of these be remittent as in Jamaica, or continued as in Barbados,—and not to be more peculiar or less indigenous to the latitudes in which it commonly occurs, than the common continued fever is to this country.

The form of Yellow Fever, or that attended by black vomit, which has been assumed by some as the only true model of the disease, acquires this pre-eminence over the ordinary fevers of the West Indies from the presence of the European, whom it attacks with a frequency and certainty in proportion to the shortness of his residence. In the absence of such subjects, the advocates of the Bulam being a distinct disease, might safely be challenged to produce any considerable number of cases exhibiting the peculiar group of symptoms and mode of termination which, in their opinion, are sufficient to distinguish it from the ordinary fevers of the country, and to mark it as an essential and separate disease.

Sickly or epidemic seasons, involving the whole population, occur in the West Indies, as in Europe, and very often at the same time of the year, and in these the new comer shows with more certainty his appropriate and peculiar grade of the disease; but it is very certain that ordinary or healthy seasons to the

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\* Dr. Burrell was the only member of the Board called on by the Lords of the Council to state the reasons on which the opinions delivered by him on the occasion were founded.

natives are often to him the reverse ; his constitutional susceptibility appearing to compensate the want of power in the cause ; and in consequence, many of the partial and irregular outbreaks of the disease are confined to this description of subject.

It would appear, the eruption of black vomit fever in the West Indies does not there, as in Europe and the more northern States of America, require always the same, or nearly the same season, or that occasional epidemic influence which seems to be necessary to its wide-spread invasions of countries beyond the tropics, and nearly, if not always, at the same season. In other words, while there are causes almost constantly present, capable of producing the disease with new comers in the West Indies—not to be explained by the importation of contagion—some occasional and peculiar constitution of atmosphere would seem to be indispensable to its development in Europe.

Yellow Fever, like the fever that devastated the Imperial army in Hungary, the Walcheren fever, and other well known outbreaks of disease among troops, has always been the scourge of armies, leaving the natives comparatively unscathed ; the intense cerebral and gastric disorder, the hæmorrhages and other formidable symptoms, and irregular and uncertain invasions of the disease, having usually, like them, been reserved for the stranger ; and I conceive we are here forced to one of two conclusions, either that the susceptibility of the stranger is the occasion of the more intense operation of causes producing in the natives a very mitigated disease, or that, for some inscrutable end, he alone is singled out as the victim of a malady depending on causes not only specifically different from those affecting the natives, but as fluctuating as his migrations. Is it credible that the black, and even the bulk of the coloured population in the West Indies, who suffer from putro-adynamic fevers, and all other diseases assailing the white races, should not in some form present a disease indigenous to their country ? To say they have it in their youth, and in a much milder form, is to fall back to the ordinary fevers of the country, and to relinquish the only diagnostic—the black vomit, by which any line of demarcation can be drawn ; in fact, to admit that the mild continued, or remittent, are the forms in which the natives present the Bulam.

The coloured population, and the acclimated European, rarely present the disease in the intense form in which it attacks the new comer ; they usually only suffer from the remittent or intermittent, and are sometimes entirely exempt at periods when the stranger is the solitary victim of the Bulam ; a fact replete with instruction, if the blindness of theory would permit us to apply it. Nothing, I am convinced, can explain this anomaly, but the greater susceptibility of the stranger to the noxious influence of a climate to which the others have become assimilated, the opportune arrival of contagion at the period he is known to be

most obnoxious to the baneful effects of that climate, being a conception too improbable to be entertained.

We appear to have selected the disease of the stranger, who is the least suited to represent the diseases of a country, either in the character or degree of their symptoms or pathological conditions. We should not consider as a fair representation of the diseases of this country, and their fatality, the results which might follow the exposure of the natives of the tropics to its cold, and vicissitudes; why, then, should we take our distinguishing mark, our point of departure in diagnosis, not from the essential features of the disease as it occurs in the natives of the West Indies, but from an occasional symptom, the exudation of dissolved blood in the stomach, which is scarcely known, in an epidemic form, but with Europeans? The wide spread and malignant fever that assailed the troops at Walcheren, would be no measure of the forms in which it attacks the natives. Let us not believe there is one cause and one disease for the stranger, and another for the native; they suffer under very intelligible modifications of the same disease. "The French," says Dr. Fergusson, "have defined it (the Yellow Fever) in one word, '*la fièvre Européenne*;' well for us had we stuck to this true definition, for it would have saved an infinity of controversy, panic, and delusion."

We are told by McLean, that so new was the disease to the French physicians in St. Domingo, that they ascribed the mortality to the ignorance and inefficiency of the English doctors. The ultra-contagionists say that the disease spread to the several islands after the arrival of the ship "Hankey," and the outbreak of the disease in Grenada in 1793, when, in fact, as subsequent sad experience has proved, this coincidence was entirely attributable to the diffusion of fresh European subjects direct from England; and such has been the almost uniform history attending the influx of new comers, whether of the army or navy, up to the present day.

Of 30 regiments that arrived in the Windward and Leeward Islands between 1816 and 1848, 10 were attacked with black vomit fever a very short time after landing; 2 within three months; 11 within twelve months; 5 within two years; and 2 within three years of their arrival. Of 13 regiments which landed in Jamaica between the years 1816 and 1834, 4 were attacked within six months; 7 within twelve months; and 2 within eighteen months. From 1838 to 1848, 7 regiments arrived in that island, but the emancipation of the negroes permitting the troops to be quartered in the mountains, a few cases only of black vomit fever appeared, within that period, in two of them soon after landing.

Between 1834 and 1838, no new regiments arrived in Jamaica, and during this interval there were very few black vomit cases,

and those chiefly among recruits. Of 40 regiments in the Windward and Leeward Islands, between 1816 and 1848, I can only find 10 which have not suffered from the black vomit variety of Yellow Fever, to a greater or less extent; none escaped in Jamaica which were quartered in the lowlands. In both commands, out of 53 regiments, 33 were attacked with black vomit fever within twelve months after their arrival; showing pretty accurately that we do not require the importation of the disease, but the arrival of the stranger for its development at almost any time or season in the West Indies. I have good grounds for stating that sporadic cases of the disease are of nearly annual occurrence among new comers in Jamaica, and in pretty accurate proportion to their numbers, and their indiscretion as to exposure, fatigue, and excesses of various kinds; causes, it may be remarked in passing, very unlikely so repeatedly to call into existence the alleged specific contagion, from which alone Yellow Fever has by some been considered to arise. Instances of the very general suffering of new comers to the West Indies could be easily multiplied, but the fact is too notorious to require further illustration.

Cases of the ordinary endemic fevers, whether the bilious remittent, or continued, are very constantly found existing with Yellow Fever, in greater or lesser numbers, and up to the appearance of what are considered mortal symptoms; passive hæmorrhages and black vomit; those terminating in this way are either with difficulty, or in some instances not at all distinguishable through one half, or sometimes three-fifths of their course, viz., the stage of excitement, from many cases of the ordinary fevers.

In what are called sickly seasons in the West Indies, the ordinary fevers are often found to precede, accompany, and follow invasions of Yellow Fever, and cases of both varieties may be found at the same time, in the same community, and even in the same family, under parallel exposure and other hygienic conditions save one, the unassimilated European constitution; the best test, sometimes the only infallible one, of the presence of causes productive of the Bulam; and it is opposed to the general history of medicine and of epidemics that two fevers, up to a certain point of their course, so nearly approximated in symptoms, in their rise, progress, and decline, should be considered as radically dissimilar in their cause and essence.

Yellow Fever, and the more severe varieties of fever, in all places north of the tropic of cancer, are as decidedly gastric, as the fevers of the eastern hemisphere are cerebral, in their character; yellowness of surface, hæmorrhages, and often an unclouded intellect are also as common and peculiar to the one, as the very general absence of these are to the others; and it appears to me, though practically warranted, we should be

pathologically as little correct, in attempting to draw a line of separation at every unusual and excessive amount of cerebral affection and mortality in the one, (which has never been attempted) as in excluding from their equally legitimate family, an occasional series of cases remarkable for high gastric disorder, in the other. These are climatic peculiarities not to be transplanted, and furnish, it may be here remarked, a much more intelligible explanation of the restriction of Yellow Fever to certain geographical boundaries, than that ships never proceed direct to India from the latitudes of this disease. The practical physician in the East has never sought a separate nosological position for any grade of fever, however differing in form and fatality from that usually prevailing, and nothing, it appears to me, could ever have elevated black vomit fever into a distinct disease, but the circumstance that the doctrine of contagion would not accord with the class of fevers with which it is so obviously allied.

To regard the Bulam as a distinct disease is a postulate quite indispensable to carry out the doctrine of Chisholm and his followers; but I question how far it is tenable with those who refer the disease to local causes. Here there is still assigned to it a generic peculiarity, not less decided than that assumed by the contagionists, and not to be maintained without inferring a distinct and separate local cause from that producing the ordinary fevers—a cause as fluctuating and contingent as imported contagion; for although an epidemic, or some superadded influence, may be necessary to account for such wide-spread invasions as those of Spain, &c., we cannot doubt, when we find the civil population entirely free, as in Barbados, in 1847-48, and on several other occasions, that the disease may spring up among new comers from ordinary causes, without the apparent aid of any such influence.

On the supposed universally continued type of the Bulam, its more concentrated form as a whole, its more rapid course, and generally greater mortality; the frequently pale colour of the liver, and the termination in black vomit, which has been exalted into a pathogonomonic symptom by some, (all, in my opinion, entirely reconcileable with a more intense febrile action in the description of subject to which in the West Indies it is almost peculiar) chiefly rest the grounds for the attempted elevation of an occasional grade into a generically different disease.

Black vomit, which must be looked upon as a peculiar termination, not a symptom, for it does not occur till a late period of the disease, is the principal characteristic of the Bulam; and the absence of which is sufficient with some to exclude all other forms, however closely approaching it in all the leading features. Thus, as Dr. Bancroft says, “selecting” a form of the disease almost peculiar to the unacclimated, and attempting to assign to

the most varied of fevers, which, according to Chisholm, would require the "fidelity of a Claude Lorraine to delineate," a character more defined and circumscribed in its phases and phenomena than plague or typhus; and yet so little specific or uniform is it sometimes in duration and mortality, that after exhibiting its usual symptoms and rate of mortality, we find it changed, as in the ships "Hussar," and "Chichester," to a comparatively mild disease, marked by a long and uninterrupted succession of recoveries, and that simply by a lower temperature, which has never been known so completely to change small-pox, or any other contagious disease, with which it has without the slightest foundation, in my opinion, been compared.

The appearance of black vomit during life occurs more frequently and in greater quantity in the Yellow Fever of the West Indies, than in that of Gibraltar; for according to Dr. Gillkrest, of 190 cases of the epidemic of 1828, only 6 vomited this matter during life. The disease altogether would appear to be more rapid and malignant in its course, in the West Indies, than in Europe. The affection of the head, and the irritability of the stomach are occasionally found to alternate to some extent with each other, and where the former predominates much, the black vomit is sometimes absent. Dr. Davy, in a note to Dr. Blair's late work on Yellow Fever, alludes to an epidemic in Barbados, in 1811, in which the gastric symptoms were very inconsiderable, and the fever at the Island of Edam, described by Dr. James Johnson, which approached the Yellow Fever in some of its symptoms, may be noticed as another of those occasional deviations from the forms common to certain latitudes.

About the rapidity of convalescence, insisted on by Sir William Pym, there is great difference of opinion. M. Louis, and the majority, state it to be generally rather tedious in proportion to the severity of the cases, several of which do not exceed a mild synochus. With such cases, I may here remark, occurring, as they generally do, after a short residence, the absence of sequelæ, or visceral disease, may be readily understood, and cannot, in my opinion, be considered as any proof of a disease different from the remittent, which is not always a marsh fever, or necessarily followed by tedious convalescence or sequelæ, as stated by Sir William Pym.

The leading symptoms of the Bulam, and those of what is called the malignant remittent, closely approximate; they differ in the more persistent and intense character of those of the former collectively; but they are chiefly, and sometimes only distinguished by the occurrence of black vomit among the ultimate phenomena of the Bulam; a contingency, or as it has been more happily termed, "the accident of a season," not, in my opinion, essential, or sufficient to disjoin them as radically dissimilar; the cerebro-gastric affection, the yellow suffusion in more or less

intensity, the irritability of stomach, suppression of urine, hæmorrhages, with occasional dark-coloured vomiting and dejections, proclaim, in my opinion, the malignant remittent, as being closely allied to the Bulam; and like it to be frequently the highest grade, conditional to circumstances of locality, of subject, and degree of cause; concurrent differences in these being equal, as in fever everywhere, to effect endless modifications.

With the exception of a pale, orange yellow, or nutmeg colour, and according to O'Halloran and some others, a dry or exanguious condition of the liver, appearances by no means constant, there is not a morbid alteration in the Bulam, that is not found in what the ultra-contagionists would call the malignant remittent, not approximating merely, but in absolute amount, sufficient, in the absence of black vomit in the stomach, to annihilate all means of distinction.

Remittent fever, the most common type in all intertropical regions, is not necessarily the product of marshy, or even of humid localities, it occurs where marsh cannot be suspected, and is not necessarily a grade of, or followed, as some assert, by intermittent fever or ague. On the contrary, the remittent is found in places where ague is never met with; thus showing the fallacy of attempting to connect, as a rule, the type of fever with the surface soil, or humidity of a place; Kingston, Up Park Camp, Port Royal, and according to Fergusson, the volcanic surface of Bailiffe, in Guadaloupe, often giving rise to the same kind of fever as that at Spanish Town, Falmouth, Montego Bay, and other localities, notoriously paludal in their character. As far as relates to ague, we are able to connect periodicity in fever to some extent with swamp; but here we must stop: the remittent type being nearly universal in all intertropical countries, whether dry or humid.

In protracted cases of acknowledged black vomit epidemics, distinct remissions are frequently observed; and cases presenting the remittent form frequently precede, accompany, and follow cases of the Bulam in the same regiment, under similar hygienic conditions. Without therefore maintaining that the Bulam is always of remittent type, it is fairly to be inferred that it is frequently so, and only fails to show remissions from the violence and rapidity of course of the disease.

The form and train of phenomena marking the Bulam are often grafted on pure remittent, and intermittent fevers, more especially in hot and humid places—a fact corroborated by nearly all whose field of observation has lain there; and it would be, in my opinion, in the present state of our knowledge, to assign a very subordinate agency to so powerful a febrifacient cause as malaria to call this a complication; it is too constant in some localities to be accidental. On the other hand there are equally

accurate and unbiassed observers who maintain that the Bulam is always continued in type, and so strong and irresistible to me are the facts in support of both views, that I am forced to believe that the type of the Bulam varies in different localities, and is not essential to the disease, or, at least, not to the production of the ultimate phenomena by which it is characterized.

Dr. Bone, Inspector-General of hospitals, who had long and extensive experience in the West Indies, speaks of what he calls the occasional "marsh basis" of Yellow Fever, and Dr. Chisholm must have observed a good deal of the remittent form in his "Nova Pestis," when he called it a compound of that and typhus. "Lemprière describes a remittent Yellow Fever." But on this point I consider the following conclusive: "I have just stated," says Dr. Bartlett, of Transylvania University, in his work on Fevers, "that Yellow Fever, like other diseases, prevailing in malarious regions, may sometimes assume something of a periodical character. This subject deserving of further investigation has recently been studied by Dr. Lewis, of Mobile. He has described a form of the disease which he calls remittent, and intermittent, Yellow Fever. During the epidemic of 1843, at Mobile, simple remittent fevers prevailed extensively in the southern part of the city, mostly among the natives and acclimated population. Dr. Lewis says that he attended in this district of the city 16 cases of the remittent, or intermittent fever, assuming the rank and grade of Yellow Fever. These cases were all among the unacclimated. Dr. Lewis estimates the number of these cases, during the epidemic of 1843, at 100, 50 of which terminated fatally. He says, the intermittents were more fatal than the remittents. With the exception of the periodical element, the disease in these cases did not differ from the ordinary unmixed forms of Yellow Fever; it went regularly through its several stages, terminated in its usual manner, and at its usual periods."

"Of 28 cases of fatal intermittent Yellow Fever, all terminated within the seventh day from the initial chill. Dr. Lewis does not give any full description of these cases, but there is no reason whatever for doubting the correctness of his conclusions. He is a competent and trustworthy observer, and he is in no way influenced in his opinions by preconceived prejudices or notions, since he recognizes, without any qualifications, the essential dissimilarity of periodical and Yellow Fever. In another paper, Dr. Lewis mentions particularly seven cases occurring in 1842, which he calls congestive, simulating Yellow Fever. They occurred in persons who had been living in malarious regions, and were marked by the symptoms of congestive and of Yellow Fever."

Dr. Lewis says:—

"The pathological appearances of the congestive fever of the

interior, and the Yellow Fever of Mobile, were both apparent in these cases, so that, taken in connection with the symptoms before death, they constituted a perfect example of the blending together of the different febrile poisons, so as to produce a disease of mixed character.

“Dr. Dickson, formerly of Charleston, South Carolina, now in the University of New York, admits explicitly and distinctly the existence of this modified form of Yellow Fever :”—

“In the summer of 1817,” (he says), “many northern and foreign sailors had been induced to go as boatmen up our rivers. Considerable numbers of them were brought into our hospitals with country fevers both remittent and intermittent, which as soon as Yellow Fever became prevalent, ran into that epidemic; the fever becoming continued, and black vomit ensuing.”

It is very evident that the causes of Yellow Fever are present in places of very different character, not only as regards the temperature and humidity, but in vegetation, soil, and the commonly supposed sources of telluric miasm. These differences apparently lead to marked varieties in the forms of the ordinary fevers, and if these are allied, as I believe they are, to the highest grade or Yellow Fever, it is questionable how far the disease is everywhere the result of the same cause, as has been insisted on by Bancroft, Fergusson, and others, or whether different causes, such as may be supposed to exist in places so different as Gibraltar; Jamaica; Brimstone Hill, in St. Kitts; and on board ship; may not in fever as in the phlegmasiæ, give rise to similar ultimate effects; for I think nothing can be more certain, than that the most perfect representation of some forms of Yellow Fever is to be found in the Algid variety of intermittent, a form of fever which Gibraltar is considered incapable of producing.

There are but two uniform and appreciable conditions which we can connect with the eruption of Yellow Fever in the West Indies—heat, and the unassimilated European constitution. All further attempts to connect the disease with soil, surface, dryness, humidity, or sources of malaria, fail to assist us; for wherever soldiers may be placed, whether in the driest locality, or in a marsh, or under the most favourable circumstances as to barracks and discipline, they rarely escape the disease.

I have shown that high gastric disorder is the prominent characteristic of all severe fevers of certain latitudes, as marked cerebral determination forms that of the fevers of Ceylon and the whole continent of India, and the irreconcilable contradictions and anomalies with which the subject of Yellow Fever is beset, would seem to point to the conclusion that fever once excited, climate, the latitude of that climate, and the other conditions under which it occurs, rather than the continued operation *per se* of any uniform specific cause, tend in some of its more concen-

trated and fatal forms to a common and similar train of ultimate phenomena.

We can scarcely, I think, doubt that efficient and similar elements for the production of fever are to be found in all inter-tropical countries, and yet, with slight occasional deviations, how constantly it takes on the form common to the latitude in which it occurs; and nothing confirms me so much in the opinion of Yellow Fever being a grade, as its absence from the eastern hemisphere, and its close resemblance in the organs affected to all the severe fevers, especially those of malaria, in all places of a certain temperature north of the tropic of cancer.

In the absence of the European constitution, the Bulam form of Yellow Fever would probably be as unfrequent in the West Indies, as in Europe, and that in a much less concentrated form. But the occasional absence of the highest and most fatal grade of a disease, will not disprove the presence of more mitigated forms of that disease, and of the causes on which it depends; and I conceive we may, with equal justice, maintain, that common continued fever and its causes are absent from this country, because we have not at all times those wide spread, destructive, and peculiar forms of the disease, which occasionally show themselves, as that Yellow Fever does not admit of any lower grade than that marked by black vomit.

Fever differs in grade and in form, and often in type, in different islands of the West Indies, and in different localities of the same island, and the features of one are so often blended with those of another, as almost to defy classification, proving the truth of Dr. Percival's remark, "that those who are most familiar with the aspect of fever, on the large scale, will be least disposed to subdivide it into genera."

In 1834 in Malta, the regiments in garrison had more than the usual number of fevers, all of a remittent character, and in two of them from 20 to 30 cases occurred, presenting great irritability of stomach, yellowness of surface, hæmorrhages, suppression of urine, and death in several cases. In the other three corps, there was not a case of this kind, nor a single death. Now will it be contended that these cases were a totally different disease from that affecting the majority, not only of the garrison, but the majority of the two regiments in which these peculiar cases occurred. In the late epidemic in Barbados, the 72nd Regiment\* was supposed to be free from black vomit fever for seven months; while two other corps were suffering from that form of disease in the same garrison, because, during that period, they had only a few cases, monthly, of a mild fever without a death. Now I ask where is the proof that these mild cases were not identical with the fever, which, from a more

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\* See Dr. Gillkrest's Report, Addenda K., p. 226. [G. B. H.]

intense degree of the same cause, was proving fatal in the other regiments. The symptoms essential to fever are few, and the Bulam, according to Sir William Pym, cannot in the beginning be distinguished. The absence therefore of the ultimate phenomena, and death, which depend on adventitious causes of aggravation, will by no means disprove the identity of the two fevers; and I am the more disposed to this opinion from the circumstance, in many of the epidemics of Yellow Fever, of cases of great malignancy coming from certain suspected places, or crowded rooms, and mild ones from others, as if inconsiderable local differences were sufficient to exalt a very mild fever into the most concentrated and fatal.

With a knowledge of the kind of subjects in whom the Bulam, and the remittent, occur respectively, are we to include in the category of the former, those cases only, that by peculiarity of constitution, or greater intensity of cause, occasionally stand out in relief by increased mortality and a single superadded hæmorrhage? I think not. No disease, Dr. Gillkrest says, has a wider range in symptom, and none, I may add, is less uniform or determinate in its attributes generally, than Yellow Fever; the epidemics of different years, and in different places, and the same epidemic at different periods, varying considerably, not only in symptoms, in degree of malignancy, and rate of mortality, but even in form and type. This has been more especially exemplified in some of the invasions of the disease in Jamaica, and the Mediterranean, in which last place, at the outset of epidemics, great diversity of opinion has prevailed as to the appearance of any new disease, and which, at length, seemed to be decided by the increasing number of cases, rather than by any marked difference, for a time, from the ordinary fevers. This, while showing the similarity of the disease with the ordinary fevers, proves a much greater capability of modification and grade than some will admit it to possess.

The year 1825, one of universal sickness in Jamaica, as well among the military as the civil population, offers much deserving of notice, not only as to the varied character of Yellow Fever, and its close approximation at times to the remittent form, but the modifications it undergoes by locality, length of residence, and weather.

The 77th Regiment stationed at Stoney Hill, and 11 months in the island, became sickly in the beginning of February, and the fever is described at this period by Dr. Richardson, as follows:—

“The first symptoms of the disease were violent headache, commonly confined to the fore part of the head; severe pains in the lower part of the back and loins, and also in the limbs, especially in the calves of the legs; great prostration of strength, anxiety, restlessness, and not unfrequently nausea and retching, in some patients of a colourless, in others of a greenish fluid.

"The skin was hot, face flushed, eyes florid, *but not watery*, nor *having the peculiar expression so remarkable in the disease some months after*; pulse pretty full and frequent, from 96 to 120. Hitherto the appearance of the tongue was various, in some patients it was clean and florid from the commencement, in others covered with whitish or yellowish mucus; the bowels usually constipated, the urine highly coloured, and scanty, but in some quite limpid and inodorous.

"After the first 12 or 14 hours there was generally an amendment of all the symptoms, or more properly speaking, *a considerable remission*, but this was of short duration. An *exacerbation* of fever soon followed with a return of headache, but not severe after the first paroxysm was over.

"The worst symptom, if it had not been present from the beginning of the disease, now supervened, viz., irritability of the stomach, and unless the febrile symptoms were again speedily relieved and subdued, this symptom rapidly advanced till disorganization of the stomach was effected, when the case soon terminated in black vomit.

"I have already stated that, the first case of the epidemic was admitted into hospital on the 1st February, and from that to the 21st of the month, the number of admissions amounted to 35, of which number seven died. During this period the disease was one of considerable excitement and of *pure remittent type*.

"On the 22nd February seven cases of fever were admitted into hospital, all of them very bad, and from this date up to the 26th March, the admissions continued at the rate of six a-day on an average, so that in the short space of 33 days we had admitted 198 men into hospital, besides an equal proportion of officers, women, and children.

"During this period the epidemic continued to preserve the *remittent type*, but the remissions gradually became shorter and less distinct as the dry season advanced, and the symptoms *progressively* assumed a more dangerous character.

"The fever now was not one of excitement, but quite the contrary, great nervous depression marked its commencement and progress, and it was evident the first influence of the miasmata was exerted directly upon the nervous system. The attack became more sudden, and it was no unusual occurrence for a man who felt perfectly well on going to bed, or in the morning at breakfast, to be prostrate with the disease in less than half an hour."

Here Dr. Richardson gives an admirable description of the symptoms usually attending the most aggravated form of the disease, and continues as follows:—

"The pulse, in some instances, I have found preternaturally slow, but the general state of the pulse, on admission, was small, soft, and frequent, numbering from 100 to 130, but rarely giving much resistance to the finger on pressure.

"The tongue in the worst cases was clean and dry, but in others rather moist and covered with whitish or yellowish mucus towards the centre.

\* \* \* "A general torpor and derangement pervaded the secretory organs and membranous structure, especially of the stomach, where a disorganizing principle was always apparent, and indicated by

a painful burning sensation in that organ at an early period of the disease.

"Irritability of stomach is a very common symptom in endemic fevers, in that, under review, it was an invariable attendant.

"The proportion of mortality during this period was one in four of the number admitted.

"From 26th March, to 10th May, 80 cases were admitted; the mortality in these admissions was 21.

"While the dry weather continued the epidemic gradually increased in violence, becoming more of the continued type; the heat more ardent; pulse weaker and more frequent; a greater degree of debility from the commencement of the disease; \* \* \* the tendency to the dissolution of the stomach greater, which was indicated by the state of the tongue early assuming a leaden, scammony colour, and gradually changed to black. Hæmorrhage from every orifice was *now* a frequent symptom, and very often carried the patient off when fever had been subdued. The disease was now more rapid in its progress, and the system could, with more difficulty, be affected by mercury.

"About the middle of April the rains began to fall, and increasing about the middle of May, the epidemic disappeared, and from the 11th of the month to the 1st of June, there was not a death in the hospital at Stoney Hill.

"From the 1st to 18th July, there were only four deaths from fever, so that from the 11th May, to 18th July, the deaths from fever only amounted to eight, and two from dysentery, thus proving the salutary effects of rain at Stoney Hill, and it was not until after the continuance of six weeks dry weather that we began to be sickly again.

"The number of admissions from 18th July, to 8th September, (of fever) amounted to about 200, of which number 57 died, the greatest proportion of mortality experienced throughout the year. During the month of August the disease was of a more ardent character, and of continued type; \* \* \* the disease now ran its course more rapidly, and was more uncontrollable than at any former period."

The above will show how little defined Yellow Fever is, either in grade, symptom, type, duration, or rate of mortality, and the power of weather and season to modify, arrest, and renew, its virulence. I will now allude to the sickness in the 33rd and 50th Regiments in the same year, the former three, and the latter six years, in the island,

Dr. King, acting surgeon, 33rd Regiment, states,—

"Five hundred and fifty-five cases of bilious remittent have been treated since the 20th December 1824, of which number 66 died, making an average of 1 in 8·4. Since the 33rd disembarked at Port Henderson on 29th September last, no fewer than 223 cases have been treated in the hospital at Spanish Town, and of which 43 have died, making a proportion of 1 in 5½.

The disease generally assumed more the appearance of typhus than usual, and exhibited all the varieties of intermittent fever. A remarkable difference might be observed when quartered on the north side. There the disease was frequently combined with arterial excitement, and ushered in with general synocha, whilst, on the contrary, the

fever at Spanish Town from the very beginning assumed a typhoid type. There was, however, great variety, both in its progress and mode of attack. Two forms of fever might very often be observed, the tertian, the quartan, and sometimes quotidian, but the usual form was bilious remittent.

“When the remissions became less remarkable, the febrile anxiety and restlessness increased; the patient is unable to sleep and complains of thirst, which nothing will allay. Either vomiting or delirium was to be dreaded, and it is difficult to decide which is the most unfavourable; when the latter was accompanied by fever, it was generally a less fatal symptom.

“The paroxysms generally ran into each other, and the disease assumed the form of continued fever, the symptoms all becoming aggravated. In this stage delirium, or coma, generally set in, and the delirium frequently became so furious, that it was found necessary to confine the patient in bed. The skin in such cases was dry and hot; pulse small and very quick; tongue hard, dry and brown, or covered with a black tenacious crust, occasionally red, resembling beef-steak.”

“When the stomach was affected, which occurred in the greater number of cases, vomiting became excessive, and everything was immediately rejected, whether in the shape of nutriment, or medicine; the skin assumes a dark brownish hue, afterwards changes to a livid or blue colour. These fatal cases have taken place every day, from the third and fourth, to the eighth and fourteenth day, but most commonly occurred between the fifth and eighth day.”

The 50th Regiment at Spanish Town, six years in the island, which had suffered from the black vomit variety, a few months after their arrival in 1819, at this time laboured under a somewhat similar fever to that in the 33rd, and in a strength of five companies, had 378 cases, and 80 deaths, which generally occurred from the fifth to the eighth day.

The period of residence of these several regiments, implying a different constitutional susceptibility, and disease of organs, consequent upon residence and former fevers, will, I think, sufficiently explain the modification in their respective diseases, without supposing a separate and distinct cause. For none, but those who contend for this, knowing the almost uniform eruption of black vomit fever, among newly arrived regiments in Jamaica, will doubt that had the 77th been placed in either of the stations of the 33rd and 50th this year, the same description of fever would have assailed them as that from which they suffered at Stoney Hill. In the one we have the dissolved blood pouring itself out in hæmorrhages and black vomit, leading, as has been suggested to paleness of the liver, and an apparently normal condition of the spleen; in the other, we find both these organs dark, engorged, and enlarged, with “the portal vessels distended with dark fluid blood,” differences, taken with the symptoms during life, which the pathologist will be able to interpret and reconcile as very intelligible modifications of the same disease. In the one we have a more dissolved state of the blood, or a less

arrest of it in diseased organs, giving rise to hæmorrhage "from every orifice;" in the other, we find the circulation struggling to the same termination, but, as in the malignant remittent generally, more determined to the brain; which in the Bulam is sometimes very little disturbed, the stomach appearing to be generally the centre of the mischief. The predominant affection of the one or the other of these organs, not only occasions marked differences in the fevers of the West Indies; but constitutes, according as it may prevail, the great leading difference in all intertropical fevers, which will be found to approach to, or recede from, the character common to the country in which they occur, in correspondence with this more than from, in my opinion, any essential difference in their nature.

We have here, in the fever of the 77th regiment, an alleged specific, defined and contagious disease, varying from excitement to great nervous disturbance, from the remittent to the continued type, from symptoms differing little from those of the endemic remittent, to the most concentrated form, and that apparently by increasing heat and changes of weather, which may modify, but can never, as here, so change the character and arrest the progress of a distinct and contagious disease. The advocates for the distinct nature of the Bulam, may contend there were here two diseases; but the transition from the one to the other, was too gradual; the susceptibility of subject, and other conditions too much on a par, and the influence of atmospheric causes too palpable to give any support to such a notion. But this is only one of the many instances of the remittent becoming exalted to the Bulam; for though the latter frequently bursts forth at once in its utmost malignancy, there are other occasions in which it seems gradually to rise from a very moderate continued, or remittent fever, to one of great intensity. Cases of the ordinary fevers are now, and again observed in the most malignant epidemics of the Bulam. In the year 1827, when the 22nd regiment, at Stoney Hill, in Jamaica, lost 122 men and 7 officers, in about two months, by black vomit fever, which attacked them about eight months after arrival; "the fever," says Surgeon Owen, "was sometimes prolonged to 10 or 12 days, but considerable remissions took place in these cases; relapses were very frequent, and generally fatal." I notice these cases as from their occurrence in black vomit endemics, with fresh subjects, they are less open to question; but the fact of the frequent remittent character of the Bulam, in many instances, is too generally confirmed by medical officers, who have seen the disease in the more humid islands, to leave any doubt of its more than accidental connection with the ordinary fevers, and their causes.

I believe the cause of the aggravation and increase of the ordinary fevers, and their assumption of the Bulam form, to be always an epidemic constitution of atmosphere in the Mediter-

anean, and the northern States of America, assisted by malaria, in its extended sense; and that these equally obtain in the West Indies, and all other places, as far as relates to the native, and other acclimated residents; deviations from this depending for the most part, not on the arrival of ships, as is alleged by the ultra-contagionist, but on a more intelligible contingency, the presence of the unassimilated European, under some excess, to him at least, in the ordinary causes of disease.

I believe *marsh* miasma not to be an indispensable element in the production of Yellow Fever, which would appear to originate from some increase in the ordinary febrile causes of whatever kind, and to obey the influence of climate, constitution, and the other conditions under which it may happen, rather than the action of any single, uniform, or specific cause; in other words, that continued, remittent, and sometimes intermittent fevers may become exalted to the Yellow Fever, and derive their resemblance to each other, not from any peculiar exciting cause, but from the tendency of all intense febrile action, to take on the characteristic affection of organs common to the fevers of the latitudes in which they occur.

I believe the Bulam to have no specific character, or pathognomonic symptom, not to be defined in its course, duration, or other attributes; but an occasional variety of a numerous and protean class of fevers; continued, remittent, and intermittent of certain latitudes; and to differ from these only in violence, rapidity of course, and ultimate phenomena; its apparent causes, its leading and essential symptoms, and pathological conditions, with the periods of its rise, acme, and decline, declaring it to be like them, a variety of the same genus, which, I am convinced however practically useful, cannot with any pathological correctness, or hope of reconciling the conflicting facts in the history of tropical fevers, admit of sub-division on any discoverable essential difference in its multiform species.

It may be thought I have generalized too much; I have endeavoured to avoid it, but the facts will fully bear me out. I cannot regard the Bulam as a distinct disease; for though it differs in some points from the ordinary fevers, its close approximation in some of the leading and more essential symptoms and morbid lesions, with the strong collateral evidence in favour of its identity with these in nature and in cause, force me to the conclusion that they cannot be disjoined as fundamentally different diseases. It is true we often see the Bulam so isolated and apparently so unconnected with the ordinary fevers, as to give to it the appearance of a distinct disease; but on the other hand, how frequently is its outset, progress, and decline, marked by cases of these, so nearly approximating to it, as to defy any attempt to say where the one ends and the other begins, or on any just grounds to point to a different cause. These and other

considerations strongly impress me with the conviction, that the Bulam, like occasional deviations in other fevers, must take its place as a grade. As such we can find its parallel in other climates, which we cannot do if we consider it a distinct disease, and this circumstance alone should make us pause in attempting it. For it looks more like the creation of imagination, than of nature, that a disease "*sui generis*," should only be known in certain latitudes, in certain seasons, and often in certain classes.

It seems to me that the mortality and peculiar termination of the Bulam have "frighted us from our propriety," and the exercise of our general experience; that we have not taken as our guide the fully formed disease, amounting as it frequently does, to little more, (making allowance for difference of climate), than a simple continued or remittent fever in this country, but have fixed our attention on its epidemic and most malignant form, which is the result of occasional and adventitious circumstances of aggravation, not of any morbid action, like that of the exanthemata, to run a defined and determinate course; the most trifling differences of locality, and of weather, often entirely changing the general aspect of the disease, its rate of mortality, and the phenomena from which it has been thought to derive a distinctive character. In 1843, I was in charge of a regiment at Maroon Town, Jamaica, 2,500 feet above the sea level. They arrived from the Mediterranean in March, and in June numerous cases of fever of a continued type occurred among men, officers, and others. From its outbreak in June to November, 80 cases among the men were admitted into hospital, and the symptoms are described in my report of the period, as follows:—

"At the outset rigors, nausea, and vomiting, with confusion of thought or oppression of head and general *malaise*, amounting, in several cases, to considerable general distress, anxiety, and apprehension. These were soon followed by headache, sometimes very severe; white tongue, thirst, high and well distributed general heat and excitement, with pains of loins and lower extremities; the nausea and vomiting in most cases subsiding, in others persisting with some obstinacy. The pulse was full, frequent, and generally soft and compressible; skin hot, but seldom dry, the bowels bound, great and distressing wakefulness. These symptoms were usually on the decline about the third or fourth day, and convalescence was not infrequent from the fifth to sixth day, leaving, however, in several cases, a very protracted and sometimes obstinate debility, and nervous exhaustion. All, however, eventually and perfectly recovered."

This then was, what has been not inaptly termed "the seasoning fever" of the 77th regiment in the mountains in 1843, as was the Bulam in the same regiment in the lowlands in 1825, as previously described, and which a few miles in the direction of the lowlands, would, I have no doubt, have converted into all the intensity characteristic of the Bulam. Will any one, but the advocates for the Bulam being a distinct disease, believe there

was any essential difference in these two fevers, or doubt that a change to a temperature of 10 degrees higher, and other causes of aggravation to be found in the lowlands, would have exalted the same fever, in the same subjects into a much more formidable and fatal disease, and that too with black vomit, which is neither pathognomonic, nor essential to the disease, but a characteristic termination of intense febrile action in the unacclimated in certain latitudes.

Much of the controversy in Yellow Fever, has been on the type of the disease, some contending for its remittent, others for its purely continued form. Facts in my opinion the most convincing, prove that either may be aggravated by circumstances of locality, season, and subject, so as to take on in fatal cases, the ultimate phenomena of the Bulam variety. Hence in some localities we find the Bulam and its associated fevers purely continued; in others as purely remittent, as distinct remissions in protracted cases and the concomitant fevers can demonstrate. The contagionists attempt to explain this by supposing a mixed or complicated disease; but competent and trustworthy observers will bear me out in saying, that the remittent form is often as much a part and essential to the Bulam, as the continued type, and that this supposed distinct disease is but an occasional and exalted variety of all types of fever. The black vomit fever of the swamps of Minorca, as described by Cleghorn, is as well entitled to the name of the Bulam, as the continued fever of Barbados, the violence of disease and common termination obscuring all distinction.

If I have interpreted and applied the facts correctly, the Bulam variety may be the occasional offspring of the continued, remittent, or intermittent fever, is part and parcel of one or other of these, and has no separate or independent existence. Sir William Pym says there were two distinct diseases on board the "Eclair," at two different periods, the remittent and the Bulam; I think it very probable there were two varieties of *one genus*, the remittent and continued, but I must believe that the black vomit cases, under the first, were as much the genuine Bulam as those in the second; exalted febrile action in both, leading to one and the same train of ultimate phenomena. The contagionists may select what type they please, but as the essential symptoms which must be taken from curable, as well as from fatal cases, amount to nothing very determinate, it is difficult to believe that either should be capable of reproducing itself.

*Does the yellow or Bulam fever, like small-pox, give an immunity from second attacks, except in very rare instances?*

I believe the Bulam fever, like length of residence, to give a considerable degree of security against an attack in the same form, or that form which terminates in black vomit, but little

protection against what I consider other forms of the disease, or those which attack the acclimated, and which I believe to originate from the same causes. The apparent immunity, therefore, from a second attack is not real, the same fever is liable to recur in a different form, or that of the remittent, modified by assimilation to the climate, and the disease of organs, which, with the soldier at least, would seem to follow a lengthened residence in the West Indies, and to determine important differences in the phases and phenomena of the fevers which attack him subsequent to the Bulam, or after assimilation to the climate.

As I said before, I cannot believe there is one cause of fever for the acclimated, and another for the new comer, or that causes equal to the production of the Bulam, which amounts in some cases to a very moderate fever, should be inactive and unconcerned in the subsequent fevers inseparable from long residence.

The history of fever, with regiments in Jamaica, from 1816 to 1838, has been nearly uniformly marked, first, by a wide spread and destructive black vomit fever, generally within 12 months of their arrival, and following this at a longer or shorter interval, and sometimes on the same station, the remittent form in greater or less severity and frequency, and often with a mortality equal to, and sometimes exceeding that of the Bulam. Now can it be imagined that causes equal to the production of destructive remittent fever, approaching in all the leading features, those of the Bulam, should not have been engaged in the invasion of the latter? Nothing in my opinion, can save us from the inevitable conclusion, that these fevers were different forms of the same disease, modified by differences in constitutional susceptibility, unless we believe with the ultra-contagionists, that the cause of the first was contagion, and wholly unconnected with that of the other, an admission which few would concede.

Black vomit fever seldom occurs twice in the same regiment, where there has been time for acclimatization; where it does, the ultra-contagionists by an "a posteriori" ingenuity contend that those who recovered in the first invasion could not have had the Bulam, but the remittent; and it is difficult to controvert this; for the black vomit being almost always fatal, is not likely to occur twice in the same person; but as relapses are as frequent, if not more so in the bulam as in any other fever, the non-recurrence of the disease, which they seek to establish, cannot, I think, be granted. Dr. Gillkrest states, there were 102 relapses in the fevers of the five regiments in Gibraltar, in 1828, several of them, after being considered convalescent, and out of bed. The oral and written testimony of medical officers, generally, confirms the frequent occurrence of relapses in Yellow Fever.

I concede the fact of the general non-recurrence of the Bulam, qualified by the reasons above stated, viz., that it is the fever of

the new comer,—longer residence preparing the constitution for the disease in another form, the endemic remittent,—but the principle of its non-recurrence, contended for by the ultra-contagionists, I cannot grant, and consider that this is contradicted by the whole history of fever, as it attacks the troops in the West Indies. In the statistical reports on the health of the troops in the West Indies, ample evidence has been adduced to prove that the liability to fever and mortality, increases with length of residence; and Dr. Jackson has shown that in the period of 12 years, viz., from 1803 to 1814, the mortality among the troops in the Windward and Leeward Islands amounted to 15,606; the greater proportion from fever, and that too from endemic causes, which I cannot doubt were similar to those productive of the Bulam; diversified in their effects by constitutional differences, intelligible to all, except those who would elevate the Bulam into a genus, which I am convinced it cannot claim.

Simple length of residence in climates where the Yellow Fever, has never it is said, appeared, affords to a certain extent, the same inaptitude to the Bulam (form,) as the disease itself; a circumstance corroborative of the immunity being derived from assimilation to the climate, rather than from the disease. "Eight officers," says Sir W. Pym in his notice of the epidemic in 1804, in Gibraltar "(who had been in India), belonging to this regiment, (10th), were attacked with the fever, and all recovered. Seven officers who had not been in India had the disease in so different a form, that five of them died; 400 of the men, who had been in India, were attacked with the disease, of which number only four died, and of 48 who had not been in India, 16 died," p. 25.

It is pretty certain, that the fact of a person having gone through the Bulam, frequently rests entirely upon the circumstance of its occurrence in conjunction with black vomit cases in an epidemic season; for, in those who recover, some are so slightly affected as not to present a single symptom, or group of symptoms, which can be considered peculiar to, or diagnostic of, any particular fever, and if occurring at any other time, would be designated as ordinary continued, or remittent fever. It is evident, therefore, that the contagionist in claiming the protective power for the Bulam, proceeds, to a great extent, on the assumption that the cause, as well as the fever, is occasional and contagious, and that all cases of a sporadic kind simulating the milder cases of the Bulam, in which he cannot find his diagnostic the black vomit, cannot, by any possibility, be the same fever. But let us assume that the cause of Bulam is neither foreign nor contagious, but endemic, which we have good grounds for doing, can we believe that this cause only exists in a degree equal to the production of the Bulam, and can give rise to no lower grade of fever? If we admit, which

we must do, that the natives labour under mild forms of fever, and that the inhabitants of Gibraltar and Spain are not entirely exempt from febrile attacks, although they may have gone through an epidemic—what grounds have we for believing that these mild attacks arise from a different cause to that producing more formidable effects in the unacclimated? and if we do not consider it different, but the same, which there is every reason to conclude, the contagionists are obviously ascribing to a principle in the disease, that which is attributable to assimilation to the climate, or, in other words, to an acquired resistance to the violent operation of the cause, and not to the complete exemption from a recurrence of the disease.

“In mild cases, however, of this last disease (Bulam),” says Sir William Pym, “and of which there are many, it is impossible to point out any symptoms distinguishing it from attacks of fever from any cause, and even in bad cases, until the fatal symptoms make their appearance, I may say (excepting its prevailing epidemically) it is as difficult to decide upon its real nature, as it is in the fever of small pox before the appearance of the eruption,” page 4. If, therefore, mild fevers occur among the acclimated in the West Indies and Spain in non-epidemic years, it is impossible for the contagionist to determine what are, and what are not, second attacks, and he evidently assumes the latter on no better grounds than the absence of black vomit, or, in other words, that the Bulam must be marked in some cases by that termination to determine its presence; an assumption which the advocates of local causes and non-contagion cannot admit.

### *Is the Yellow or Bulam Fever contagious?*

I believe Yellow Fever in its simple form to be absolutely and universally non-contagious.

I know of no disease entitled to be considered contagious, so dependent on constitutional, atmospheric, and local causes, as Yellow Fever is notoriously known to be; confined to certain parallels of latitude, to certain seasons, and often to certain classes, it wants that universality marking the class of specific contagions; and looking to its intimate alliance with the fevers of malaria, and the high temperature under which it commonly occurs, together with the absence of what I consider proof, I do not believe that Yellow Fever in its simple form can, by any contingency, reproduce itself.

In the outbreaks of the disease in the Mediterranean, “all other diseases have seemed to merge in the reigning epidemic,” an implied exclusion of ordinary causes more consistent with some general and atmospherical change than any suspension of these which a strictly contagious disease, more especially an imported one, was likely to occasion. There is no greater proof

to my mind of the non-contagious nature of a disease than its indiscriminate spread through a large population within a few months; and when we look to 76,000 persons attacked out of 80,000 in Seville in 1800; to 48,000 out of a population of 60,000 in Cadiz in the same year; with other destructive eruptions of the disease in Spain, where, in 1804, no less than 23 cities were afflicted, and these nearly always in the last five months of the year; it seems more consistent with our experience to believe, that some general, or epidemic cause, was at work, than that, in so short a space of time, such multitudes, with all their fears and consequent precautions, should have come within the limited sphere of contagion, which, according to Haygarth, will not reach beyond half a yard in the open air, even in the malignant small-pox.

The spread of a disease to great numbers can rarely be held as any proof of contagion, but rather the reverse, and I may safely affirm, that the acknowledged contagious diseases have never, in a given time, proved so universal and destructive as many of the epidemics of Yellow Fever.

There are no better grounds for suspecting contagion in Yellow Fever, on account of its rapid diffusion among a population, than for the fever of the British army at Walcheren, where McLean says, "On the 17th September 8,200 or more than one-half were sick, on the 22nd 9,500, and on the 23rd 9,800 or nearly two-thirds, and in one month 1,000 died." "The destruction in Hosiers' fleet in 1726 off the Bastimentos; the loss of 20,000 lives with Vernon at Carthage; the fate of the expedition fitted out from Jamaica for the Spanish main in 1780; and many similar disasters, are all in proof of the great importance of calculating upon the influence of the seasons, and the laws of epidemics;" agents, I may add, which, however undefined and incomprehensible, cannot now be questioned, except by the ultra-contagionists, with whose doctrine they cannot square. The greatest proof of their influence is the impossibility of the extension of Yellow Fever beyond its appropriate season and weather, at least in Europe.

Were Yellow Fever a contagious disease, the innumerable instances in which the freest intercourse has taken place between the diseased and healthy since 1793, would have furnished evidence sufficient to satisfy the most prejudiced, and I hold the negative proof to the contrary to be so overwhelming and decided, as generally to surround the very few suspicious cases opposed to it, with every doubt of their being either accurately or candidly related.

Millar, Chervin, Fergusson, and others, state, that attendants on the sick have always escaped where the hospitals have been placed beyond the noxious atmosphere, and Dr. Gillkrest enumerates 30 towns in America where persons carried disease and

bedding without communicating the disease. The disease did not spread on the Neutral ground at Gibraltar in 1828, though the convalescents of the 12th Regiment\* took with them their clothes and bedding; and according to Chervin and others, civilians who carried the disease there, were sick in their tents for several days, surrounded by their friends and relations, and with entire impunity to the latter. For 18 days that the 12th Regiment did not take the town duties, they were entirely exempt, and 92 women and 190 children of this regiment, several of whose husbands were attacked, escaped the disease; a result, says Mr. Amiel, scarcely attributable in those cases to the free air of the neutral ground, which is supposed to have great power of dilution by the contagionists, though much inferior in strength and constancy to the "breezes" of the West Indies in which the disease is known to rage with the greatest malignancy.

In nearly all the epidemics of late years among the troops in the West Indies, a removal to camp has been almost always successful in immediately arresting the disease, and so effectual is this known to be, that the measure is now very properly not left at the mercy of any opinion, but peremptorily required to be always carried into effect.

The exemption of the black races, and, to a great extent of the coloured population of hot countries, which can only be attributed to their assimilation to heat and the other local causes of Yellow Fever, the blacks in the Northern States of America suffering equally with the whites, I consider one of the strongest proofs of the domestic origin of the disease, and of its non-contagious nature. Mr. Doughty states, "that blacks and people of colour have, in the West Indies, seemed to have been as free from the fever as the person who has had the small-pox, is against its recurrence. The pioneers, who removed the most obnoxious matters from the sick, were never affected."

"During the autumnal months," says Dr. Lewis, of Mobile, "congestive fever prevailed so generally in my neighbourhood as to amount to an epidemic; there were, in my professional circle, two blacks to one white, yet I did not see a single case of congestive fever in a negro, nor did I hear that any died of the disease in that country. I have made inquiries of several medical gentlemen who have been long practising in the country, their experience does not materially differ from mine; the fact is, that the remarkable exemption from Yellow Fever which this race enjoys, extends, in a great measure, to all the malarious fevers of hot countries."† A remarkable instance of the latter is to be found in the exemption of the negroes in the

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\* There is probably some slight error in transcribing here; but the details may be referred to in Mr. Amiel's "Replies to Queries," p. 6. [G. B. H.]

† Bartlett on Fever.

expedition to the Niger. Staff Surgeon Lawson mentions, that though the black population in Free Town, Sierra Leone, in 1837, amounted to 14,000, and the European to 80; there were more cases among the latter, than the former of whom only three died. Even length of residence would appear to give exemption, for it will scarcely be contended that, the 3,000 or 4,000 Frenchmen who fled to Philadelphia, as stated by Dr. Bancroft at the time of the epidemic there in 1793, owed their entire exemption to former attacks, unless it be admitted that the disease is much more constantly present, and in a more mitigated form, than allowed by the contagionists.

So discriminative a property as here evinced, a property so constant, uniform, and to be calculated upon, is at variance with all our notions of a readily transmissible disease. "Places not persons," says Dr. Fergusson; for though black vomit cases are from time to time carried to the mountains of Jamaica, and put into the same hospital with other patients, no instance of its ever having been communicated is known, and the same will apply to less distant and elevated places, which, were the disease contagious, would have furnished the strongest evidence of its powers.

No less opposed to contagion is the comparative immunity of females, "the ministering angel" in all our sickness, and therefore necessarily more exposed to diseases of a contagious nature. In the epidemic of Cadiz in 1800 the mortality was 7,387, of which 5,810 were males and 1,577 females. Lygon mentions, that in the epidemic of Barbadoes in 1647, "for one woman that died there were 10 men." In the epidemic of 1804 in Spain, the aggregate mortality in 23 towns was 45,822, of which 28,352 were males, and 17,470 females. But it is unnecessary to multiply instances; the preponderance is almost constant, and the inference to be drawn is too conclusive, in my opinion, for any sophistry to set aside.\*

There can be no proof so satisfactory of contagion, as carrying the disease from the place of infection, and transmitting it to the healthy at a distance. It is here the contagionists have failed, for though thousands have, from time to time, died of the disease at short distances from infected localities, and among the healthy, the few cases they have collected, and that with much difficulty, serve rather to amuse than convince, and cannot, in my opinion, stand opposed to the facts, demonstrative of the incapability of Yellow Fever sustaining the character of a virulent and consistent contagion.

In the year 1800, says Chervin, 14,000 persons left Cadiz on the breaking out of Yellow Fever; in Alicante in 1804, 2,110 fled; in Tortosa in 1821, 5,000; in Majorca in 1821. 20,000; in Barcelona, in the same year, 80,000 persons, several of them

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\* A similar preponderance is observed in periodical fevers.

sick, migrated; and in the epidemic of Leghorn 8,000 or 10,000 persons fled to Pisa and the adjacent country, without spreading the disease.

"In the epidemic of New York in 1805," Dr. Millar says, "there was no communication of the disease in hospitals, at a small distance from the city. The hospital attendants and those occupied in the removal of the sick from the city to the hospital, and who went into the most pestilential quarters of the town, entered filthy apartments, and lifted the sick into their carriages, dressed in their foulest clothes, and sinking under the worst degrees of disease," escaped.

In the late epidemic in Barbados in 1847-8, while the 66th and artillery were more or less attacked with black vomit fever from February to December 1848, the 72nd in the same garrison, equally susceptible with the 66th, a short distance from them, and with the most unrestricted intercourse, remained free from the disease till October.

"In the month of March 1825," says Dr. Venables, of the Ordnance, "a detachment consisting of two Companies arrived from England; they were composed almost entirely of very young soldiers, who had never before served on any foreign station. One Company landed at Port Royal where it remained, the other was ordered to proceed to Up Park Camp. The Company stationed at Port Royal lost 12 men by fever in less than three months, viz., from 22nd March to 10th June, the other troops at the same time in the same garrison (three Companies, 50th Regiment, 153 strong,) lost not one man by fever, or any other disease during the same period, and yet the most free and unrestricted intercourse prevailed amongst all the troops in garrison at the time." Among the detachment of the 50th Regiment there were 50 recruits, and, of course, highly susceptible.

Dr. Gillkrest, in his examination before this Board says—

"All my experience in the West Indies in 1801, and Gibraltar, in 1828, and the result of my deepest consideration, as well as a review of its history in different parts of the world, go fully to establish the strongest conviction on my mind of its not being contagious under any circumstances whatever; that even crowding of many patients together, though liable to produce other complaints, will not produce Yellow Fever any more than ague, which cannot be produced by crowding."

In support of the above opinion, Dr. Gillkrest submitted the following facts:—

"1st. That the attacks among 69 fatigue men of the 43rd Regiment, who were employed in attending the sick in the epidemic of Gibraltar, in 1828, in the number of from 1 to 3 daily, were a fraction less than those in the general body of the regiment.

"2nd. That although 43 cases had been treated between the 12th and 28th September, none of the servants had been attacked. The permanent orderlies were in constant attendance a month before any of them

were taken ill in any of the hospitals, and not before the civilians in the neighbourhood had become affected.

"3rd. That the attacks among the washerwomen could in no case be reasonably traced to contagion; the greater proportion was not at all attacked, and those who were, were not attacked until the neighbourhood where they resided had become unhealthy.

"4th. That out of 516 individuals of families in the closest contact, 312\* were not attacked; this being a much larger proportion than in contagious diseases, in which some state the non-attacks to be 1 in 26; others, 1 in 34.

"5th. That patients with other diseases, and medical officers, were not attacked until the disease had extended to the neighbourhood, until an advanced period of the epidemic, and Mr. Frazer, 73rd Regiment, who was a considerable time in the lazaretto, was not attacked till he came into the garrison, where there was much less chance of contagion. I saw three patients in the lazaretto, and Mr. Frazer in attendance upon them.

"6th. That the military contracted the disease, though in as great isolation as it was possible they could be, having for upwards of three months, no contact with sick, or with any one who, by the remotest possibility, could have transmitted the disease to them.

"7th. That upwards of 4000 persons removed to the neutral ground with their bedding and furniture, and the disease must have spread, had it been contagious.

"8th. That a woman of the name of Ackerman was taken ill in a shed, and no less than 18 susceptible persons were in contact with her, without contracting the disease.

"9th. That Mrs. Farquhar, who had had the disease on a former occasion, cut off all communication with persons beyond her gate, on account of her niece, who had lately arrived from England. There was a space of several yards between the gate and house, so that a close approach was prevented. Notwithstanding, this young lady was attacked in the severest form and died.

"10th. That the vessels in the bay, amounting to 300, were perfectly exempt, though there was constant communication with the shore, and one medical gentleman, Mr. Mathias, was in the constant practice of visiting his family on board ship, though in daily attendance upon patients. He died of the disease."

*Is the Yellow or Bulam Fever capable of being imported?*

I am of opinion that the Yellow Fever cannot be imported so as to extend itself to a previously healthy and unpredisposed population; that the instances of the supposed propagation of the disease in this way cannot, in any case I have met with, bear, in my opinion, the test of that rigid examination necessary to determine a question of such importance to the interests of humanity and science; and that the coincidences of the arrival of diseased ships, and the eruption of the disease, which will happen as long as Yellow Fever exists, and ships sail, cannot be considered as cause and effect, but a fortuitous concurrence of events, until it shall have been first established that the disease is of a con-

\* Qy. 372. See Appendix I., p. 171. [G B. H.]

tagious nature, and capable everywhere of showing, under favourable circumstances, that character, a conclusion which I maintain rests on nothing that can be called evidence.

Time after time have black vomit cases been landed at Port Royal, Barbados, and other ports, without spreading the disease, and that too under the most favourable circumstances for its propagation were it contagious; and however this may be attempted to be explained by those who believe in qualified contagion, they will have difficulty in reconciling that a single suspected stranger in Gibraltar in 1804; a suspicion in 1810, and 1813; and the ship "Dygden," with which there had been no communication\* in 1828; should on these occasions have proved more potent than a ship-load at other times. "In 1799," says Tomassini, "the frigate 'General Green,' arrived at New York; she had become leaky from the effects of a violent storm, and having afterwards been subjected to excessive heat, the Yellow Fever broke out. The numbers of sick and of deaths were very great. As soon as she arrived, they sent 100 patients on shore; no attention was paid either to the disease, the sick, or their effects. Notwithstanding, they did not communicate it to a single person, either in the hospital, or in the town!"

According to Arejula, a fleet from the West Indies, in 1805, landed 200 sick at Cadiz, many of them with yellowness of skin, and black vomit, and the disease was not communicated to the population; and yet the destructive epidemic of 1800 in the same place was supposed to have been imported by a ship to which no other suspicion could attach than her having been at the Havannah.

Dr. Wilson states that H. M.'s ship "Euryalus," in 1819, anchored in Carlisle Bay, Barbados, from Bermuda, with fever on board; she was refused *pratique*, and went to sea. "The sickness increased, she put into the Danish Island of St. Thomas, where the governor, actuated by the ordinary feelings of humanity, gave orders for the immediate reception of the sick in hospital. Seven men were landed at first, and numbers afterwards, and it was never believed, or alleged, that they communicated the disease."

The principal medical officer in this year was a contagionist, the only one, I believe, of an ultra kind, to be found in this class for several years, and who had instituted quarantine regulations to the great annoyance of the inhabitants.

"In 1820 and 1821," says Assistant-Surgeon Dr. Bone, "Yellow Fever prevailed in St. Ann's Garrison, Barbadoes, quarantine regulations were enforced in 1820, but not in 1821, and the disease stopped in both years at the same season, the beginning of February of the following year." The same writer states "that in Tobago, in 1820, in 17 months, 99 from 144

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\* Qy. no infraction of quarantine regulations. [G. B. H.]

persons died in the garrison, although the quarantine regulations were enforced." "The contagion doctrine," he continues, "was strongly opposed by a great majority of the medical department in the West Indies, and died a natural death in 1821, when Inspector Green left the West Indies."

H. M.'s ship "Pyramus," in 1821, and the "Crocodile," in 1843, landed several black vomit cases in Barbados, without any extension of the disease to the hospital attendants or others. In both instances the cause of the disease was satisfactorily traced to the foul state of the ships, and several of the persons engaged in cleaning them out were attacked.

There can now be little doubt that the cause of aggravation, at least, of Yellow Fever, is frequently in the ship herself, and the fact of cases coming from the neighbourhood of the pumps, and other suspected places as noticed by Dr. Wilson, is corroborative of this. The source of malignancy, if not of the disease, appears to be frequently confined to a very limited space. In a detachment of troops on Monk's Hill, in Antigua, in 1817, none were attacked, except those who did duty at English harbour, although they were all treated at the former place. No fever of the kind prevailed among the people of the dockyard.

Lower rooms, married huts, and casemate barracks, have always been remarkable for the number and malignancy of cases; and the increase of sick and mortality among the crew of the "Eclair" steamer in 1845, by being placed in a small fort at Boa Vista, in the month of August, with the thermometer at 86, is in perfect keeping with the history of many invasions of the disease, in which, as in this case, nothing like contagion could be traced for their origin.

Preliminary to all the deductions of the contagionists is the assumption that the disease is contagious, and taking this as granted, they have invariably adopted the *post hoc propter hoc* mode of reasoning, and saw proof where others found coincidence, a trunk of clothes from the West Indies being with them sufficient to infect a whole population, while innumerable instances of actual contact with numbers of sick, without the transmission of the disease, have been attempted to be explained by variation in susceptibility; the absence of some necessary condition, as that of the *Ferment* of Hossack, or the predisposition and con-cause of Arejula, or to anything but that which rational induction would have arrived at, the non-contagious nature of the disease.

The advocates of the transmission of the disease from place to place, have assumed as proof of this, the long intervals of exemption, but they would require to show that in those intervals sporadic cases were not observed, and this they cannot do; for the verification of no less than 38 cases that had occurred in the few years prior to the epidemic of 1828 in Gibraltar, proves the occasional occurrence of a disease identical with what they

have alleged to be always imported. The presence of cases of Yellow Fever in non-epidemic years in the West Indies may be gathered from the following extract of a letter from a private practitioner in Jamaica :—

“Isolated cases of Yellow Fever, of most malignant type, terminating in black vomit or black dejections, present themselves every season in particular localities among new comers. The experience of several years of professional connexion with the parishes of St. Dorothy, Vere, and the low lands of Clarendon, has afforded me ample opportunities of ascertaining this fact.”

I find by the military medical reports, that cases of black vomit fever have occurred to a greater or less extent in one or more of the Windward and Leeward Islands every year from 1816 to 1847, with the exception of 5 years; and that Jamaica, from 1817 to 1838, when the troops commenced to be quartered in the mountains, was only one year exempt from such cases. This tells both ways, either for the almost constant presence of local causes for the production of the disease, or that of contagion. I leave to disputants to settle the question, I need not say to which I subscribe.

One would suppose that a disease so marked, peculiar, and differing “from all other diseases,” as the Yellow Fever is said to do, would have been immediately recognised on its introduction in any place were it of foreign origin, but so much has it simulated the ordinary fevers in Gibraltar, that in all the invasions in that garrison in 1804, 1813, and 1828, we find great difference of opinion among medical men at the outset, not only as to the identity of the disease, but to any disease different from what they had been accustomed to see frequently. In 1828, Mr. Dix saw no difference from cases he had observed annually, and Mr. Wilson, of the Civil Hospital, saw nothing unusual in the epidemic, except in severity of grade; he had seen black vomit fever almost every year, felt no alarm, and did not report. In fact, there are good grounds for believing that the presence of the Yellow Fever was determined by all but the importers of contagion, by the increase of cases and mortality rather than anything peculiar or novel in the nature of the disease.

If there be any fact well established in Yellow Fever, it is the impossibility of transporting it to the free and open country, or even to short distances from the scene of its ravages. The non-extension of the disease to Brooklyn and other villages adjacent to New York, as noticed by Dr. Millar; to the neutral ground at Gibraltar; or to the encampment of troops; prove to my mind the absence of anything like a contagious principle. The crew of the “*Eclair*,” by being placed in the fort at Boa Vista, were brought from Scylla to Charybdis, and in no similar case could we look for a different result; but the whole history of the disease confirms the fact that, wherever the physical conditions

are in any respect improved, the mortality is not only diminished, but the disease often so changed in aspect as to be repudiated by the contagionists, as something totally different to what they wish to recognise as genuine Yellow Fever. I would here ask, are the admitted specific contagions, under any circumstances, so changed as thus to lose the characteristics by which they may be identified? What were the symptoms in the early cases of the ships "Bann" and "Eclair," and in the later cases of the fever on board the "Hussar," which the advocates of a distinct and contagious disease would select as pathognomonic of Yellow Fever? The non-contagionist sees in both cases but modifications of the same disease. The contagionist, to support his doctrine, discovers two distinct and separate diseases, without being able to detect a separate and distinct cause. The probable aggravation of the fever by the well-prepared constitutions of the crew of the "Eclair," and the increasing heat of the weather; very intelligible causes of aggravation of fever, are considered very subordinate to an assumed contagion, which, under diminished temperature on board the "Hussar," cannot sustain the same character of disease, but resolves itself into a very moderate fever without a death (in about 80 cases) to proclaim its distinctive attribute, the black vomit. We have yet to prove that the alleged contagion of Yellow Fever "differs from all others in being increased by heat and diminished by cold."

It is difficult to say what disease may, by accumulation, acquire contagious properties not originally belonging to it, but looking to the rapid course of Yellow Fever, the climate, the well-ventilated hospitals where it occurs, and its frequent origin in causes wholly unconnected with crowding, and the other supposed sources favourable to infectious diseases, coupled with the fact, that contagious diseases in this country, under opposite conditions, are generally confined to a very circumscribed area—I cannot, in the absence of proof, subscribe to the modern opinion of qualified contagion, and must believe, if it has ever spread contagiously, that it was no longer genuine Yellow Fever. With fever, in my opinion, not essentially different, and a climate equally favourable to increase its contagious powers, if such a doctrine can be admitted, we have never heard of anything like contagious fever in the eastern hemisphere, and as disease is pretty equally distributed throughout the world, it creates considerable suspicion of Yellow Fever being but a grade, and non-contagious, that a disease "*sui generis*" should nowhere find its parallel, but in certain latitudes.

The supposed occasional power of transmission of Yellow Fever from person to person rests on no better evidence than that which a predetermined search for contagion would find in influenza and other non-contagious diseases. The only presumptive proofs, viz., those of communication of the disease to

persons at a distance from the infected locality, are so exceedingly rare and exceptional, and apparently so incapable under parallel circumstances of repetition, as fairly to warrant us in regarding them as the semblance of contagion, not the reality.

The alleged proofs of the contagionists are but as a drop in the bucket, compared to the instances in which the disease has failed, under equally favourable conditions, to show any apparent contagion, and were we to admit them, even under all the restrictions of the most qualified advocate of the doctrine, the whole history of the disease attests the fact, that no past experience would enable us to draw the line, or to predict the time, place, or other circumstances, under which it might develope this property; the alleged virulent contagion of one epidemic, not only failing to show itself in another, but so absolutely disproved by the occurrences, where "*cæteris paribus*" it might be again looked for, as to involve contradictions which no one but an ultra-contagionist can hope to reconcile.

The varied character of the disease, its restriction to certain boundaries, the modification it undergoes by changes of weather and locality, its selection of certain subjects, on the presence of whom, in the West Indies, even its very existence depends, must exclude it from the class of specific and perpetuated contagions; and who by any facts he can bring forward, will presume to determine at what period a fever of a remittent type, and wanting for a time in several of the more marked symptoms of the Bulam, as in that of the 77th regiment, at Stoney Hill, acquires contagious properties. Is the operation of contagion so little determinate, so contingent and variable in degree, as not only to show itself under different forms and grades of disease, but to be abruptly extinguished by rain, and renewed by dry weather, as on this and several other occasions? Is contagion so discriminative, that it should pick out, as if from the muster roll, as Dr. Fergusson says, certain unacclimated men in a regiment, leaving the older residents who may, or may not, have had fever, either exempt, or with symptoms which according to the contagionists, cannot by any possibility have the same origin, though occurring at the same time, in the same barrack-room, and under the same mode of living, discipline, and exposure?

In an epidemic, simultaneously involving great numbers, and that at distant points, can the attack of the healthy, after intercourse with the diseased, be fairly considered as cause and effect? Can the successive attacks of several members of the same family, of conductors of sick, and of patients under other diseases, or their attendants, be ascribed to contagion, in a community equally predisposed, and within the sphere of the same local causes? for local and circumscribed they are proved to be as the almost invariable arrest of the disease by a removal to a very short distance incontestibly demonstrates. Are not the same

occurrences, the same order of sequence to be found, in a wide spread remittent fever, or other non-contagious disease, and are we in the face of the strongest collateral evidence of the non-contagious nature of the Bulam, to regard these as proofs? for beyond this, beyond the immediate scene and place of its eruption and ravages, the contagionist cannot carry his proofs, and is constantly reminded of the evanescent nature of his contagion, which, though he imagines can be transported across the Atlantic, fails to afford him the slightest support, a few yards from where it has destroyed hundreds,—the sick, the dying, their clothes, bedding, and other effects, being then insufficient to originate a single case of a disease, which at other times has been supposed to extend itself to the whole of the West Indies from a bag of foul linen.

To return to the question of importation, can we be more than amused, when we are gravely expected to believe that the disease was introduced into Gibraltar, in 1804, by a single stranger, and in 1814 and 1828, by something much less tangible, a suspicion which in the latter year, with the most assiduous search of those “who wished to prove importation,” remained totally unsupported? I ask, can we, knowing the occurrence of sporadic cases, from time to time, without the extension of the disease, its frequent spontaneous outbreaks, and epidemic prevalence in other places where no such suspicion existed, and where there were abundance of other circumstances to which all experience points as intelligible causes, give any credit to such statements?

Is it the coincidence of the arrival of ships from suspected places, and the breaking out of the disease that can strengthen our belief in importation, when it is known that between 1816 and 1828, 840 vessels arrived in this bay (Gibraltar) from the Transatlantic countries, where the Yellow Fever usually prevails; and that black vomit cases have been landed again and again in other ports with an impunity which, were the disease contagious, no variation in predisposition, or the alleged protection of former attacks, the constant refuge of contagionists, could reconcile? “In 1802,” says M. Chervin, quoting from Pariset, “Admiral Gravina caused 500 patients with Yellow Fever to be disembarked at Cadiz, who were taken to the Hospital St. Juan de Dios, and treated therein, without having communicated their malady to any person.”

Is it the interval between one epidemic and another, or the previously healthy condition of a population, which can warrant the conclusion, when the same equally apply to visitations of influenza, and other non-contagious diseases throughout the world?

Is it the eruption of a new and peculiar disease unknown to the place, the season, or any other circumstance attending its invasion, that justify belief in importation, when we know that

sporadic cases are familiar to the resident medical men, and occur almost always at a season well known, throughout the Mediterranean, as the most prolific of fever in its epidemic form?

Is it the locale of its outbreak, and mode of extension, which afford grounds for suspecting its foreign and contagious nature, when it is notorious that whenever the epidemic has appeared in Gibraltar, it has always commenced in the "filthiest spot," and "that the disease did not spread from any focus, but broke out in 50 different places at once?"

Finally is there a single recorded instance of importation that can be considered conclusive? To take the latest and most authentic account of its supposed introduction in this way, that by the "Eclair" steamer in 1845, into Boa Vista; I submit there are circumstances in this history, which, in the present state of our knowledge, are calculated to raise every doubt of the disease in this island, having been in any way connected with the "Eclair."

Dr. M'William passes over no inconsiderable intercourse with the "Eclair," immediately on her arrival; the exposure of 40 labourers to the fomites (if any) of the vessel whilst engaged in cleaning her out, who returned to their homes every evening; the landing and washing of "about a dozen bags of foul linen," by 17 washerwomen;—and fixes the focus of contagion, which it is alleged spread to the remote villages, on two soldiers who had been from 7 to 11 days respectively, connected with the fort before and after the departure of the "Eclair," without contracting the disease; and though not themselves attacked for several days after intercourse had ceased between them and Anna Galinha, ("the first case that proved fatal, or attracted any notice among the inhabitants,") are yet supposed to have communicated the disease to this woman, a month after the departure of the "Eclair." These men had stripped off the only clothes they brought with them from the fort, and "were not laid up" for eight days, so that we are left to conclude the disease was communicated in its formative stage, and several days before it had developed itself in the persons suspected; for it does not appear to me at all evident according to the general mode of accession\* of Yellow Fever "*that they had the germs (of the disease) in them, by the symptoms alone which were manifest while they were there;*" more particularly in that intense form, accompanied by black vomit, which Pedro Manoel, one of these soldiers, stated he had had.

The above circumstances, together with the spread of disease among the cattle, as stated by Dr. King, who visited the island, and reported upon those occurrences, subsequent to Dr.

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\* Vide Dr. Richardson's Report, page 29.

M<sup>r</sup>William ; the unaccountable absence of remittent fever, with which a truly contagious and foreign disease was not likely to have interfered, the season of the year, and the increase of sickness and mortality of the people of the "Eclair," in the fort, which was, in every respect, well calculated to generate Yellow Fever, and sicken the guards, without the aid of contagion,—all concur to show, in my opinion, an epidemic constitution of atmosphere, and not contagion. Lind notices the liability of the Cape de Verd Islands to destructive outbreaks of fever ; and as some corroboration of this, I may mention that as medical officer in charge of troops and settlers for the Cape on board a transport, in 1819, which put into the island of St. Jago for six or eight days, I was, as also the master and others, most earnestly entreated and urged by several inhabitants, to hurry our departure, on account of the approaching season of fever.

I may here notice the objection raised by the contagionists against the supposed influence of an epidemic cause in Yellow Fever, viz., that it would not be confined to the town of Gibraltar, but extend to the neutral ground, where the population has always found protection. The occurrences in the last visitation of cholera in London, may be quoted in reply, and particularly the case of the pauper children in Drouet's establishment, at Tooting, who were moved to London with the happiest effect, thus showing they had simply removed from the conditions favourable to the development of the disease.

As long as men can view things differently, there is ample scope for difference of opinion on the subject of Yellow Fever. I believe in the present state of our knowledge, the question does not admit of being settled ; and the opposite conclusions arrived at by the several inquiries, composed as they have been of equally competent men, while showing the futility of such investigations, points out the propriety of our being satisfied, at least for the present, with the instalment which, I conceive, has been made towards its adjustment. That instalment, as far as I can discover, by a careful analysis and digest of the whole subject, supported as it is by the majority of medical officers in the army, and I believe in the navy, who have the best opportunities of forming correct conclusions, is the great probability of the non-contagious nature of the disease.

In the Report of the Commission appointed in 1827, by the Royal Academy of Medicine of Paris, to examine into the documents furnished by M. Chervin, relating to Yellow Fever, it appears that out of 531 physicians of different countries, 48 only admitted the contagion of Yellow Fever, but "*à degrés très variés et avec des restrictions plus ou moins marquées*," and of these, nine were more or less qualified contagionists, some believing it was not essentially contagious, but might become so under certain circumstances ; others that it was contagious in Europe, and the

United States, but not in the tropics. On the opinions in favour of contagion, the Commission remarks,—

“It is our duty to remark that these documents (42 in number) have appeared to us to contain but a very small number of facts, susceptible of explanation by contagion, and that they are in general wanting in circumstances which would be necessary to deduce from them anything conclusive.”

Of the non-contagionists, amounting to 483, the Commission states,—

“That the greater number had witnessed the Yellow Fever during periods of from 10, 15, 20, and 30 years, several of them 40 and 50 years, and that though the American physicians do not unanimously recognize non-contagion in Yellow Fever, there is not, perhaps, a disputed point in medicine, in favour of which it was possible to assemble an equally great majority of evidences, than M. Chervin has done on the question of which he treats; and what is truly astonishing, that he has obtained these evidences, so like to each other, from physicians of so many schools, and of so many different nations.

“But if from opinions, (continues the Commission,) we pass to the facts recorded by the non-contagionists, we shall see that they speak everywhere, a language clear, positive, and uniform, to which it appears to us difficult to withhold confidence. They have never seen the Yellow Fever propagate itself in the rural districts in the United States, although since 1793, thousands of infected individuals have gone to die in the bosom of their families. Contact, the most direct and immediate, has not communicated the disease in a single well-established case, an assertion which is repeated almost unanimously by the physicians of the different towns of the coast, visited by M. Chervin, from Louisiana to Maine.”

The Commission conclude their Report by the following remarks, which I prefer to give in the original:—

“On veut savoir ce qui est resté dans notre esprit de la lecture d'un si grand nombre de pièces authentiques dans leur forme, presque toutes, dans le sens de la *non-contagion*. En répondant qu'il en est résulté pour nous une impression favorable à ce système, nous ne faisons qu'exprimer l'opinion presque unanime de votre Commission. Après avoir pris connaissance de tous les documents qui lui ont été soumis, après les avoir lus, analysés et discutés un à un, pièce à pièce, elle pense que ces documents, en admettant comme exact les faits qu'ils contiennent, méritent l'attention la plus sérieuse; qu'ils augmentent considérablement la masse des observations favorables à l'opinion de la *non-contagion* de la fièvre jaune, et qu'ils seraient de nature à concourir puissamment à établir en principe cette *non-contagion*, si, dans l'état actuel de la science cette question pouvait être résolue.”

These opinions, coming from such men as Dubois, Double, Husson, Laubert, Orfila, Renauldin, Thillaye, and Vauquelin, who composed the Commission, are entitled to every confidence; and when we look to the mass of documents from which they deduced these opinions, collected as they were, by M. Chervin,

in every country, and with a zeal and disinterestedness almost unexampled, they contribute in no small degree to the probability which I have claimed for non-contagion. No less valuable, and tending to the same conclusion, is the collection of facts to be found in the reports of army medical officers, extending from 1816 to 1848. These are "plain unvarnished tales," written in the midst of, almost at the bedside of, Yellow Fever cases; without bias, and without stimulus, save that of a faithful and conscientious discharge of duty, their observations are deserving of all the weight and confidence which disinterested inquiry may claim. The few, comparatively, who have offered an opinion, very generally advocate that of the disease being a grade, and non-contagious, but the silence of the majority on these questions probably declares more emphatically than volumes, how little the bugbear contagion, has haunted their imaginations.

In conclusion, I am no partisan, I have written this report as I have others, simply as an act of duty, and I hope and trust in an equally inoffensive spirit. If I have spoken strongly, it has been from no disrespect to the opinions of others, for whom though opposed to their views, I entertain every deference, but from a wish that the facts before me should not fall short of that construction and application to which I think them entitled. No one who has thoroughly investigated the subject of Yellow Fever, but must be deeply impressed with the numerous difficulties which surround it, and, as equally clear-headed and competent men have adopted opposite views concerning it, no one, whatever his intellect or pretensions, is warranted in arrogating to himself, that he is "Sir Oracle" on a question never destined, probably, to be settled by universal assent. I have wished to give my opinions, however decidedly I may have expressed them, with, I hope, a humility befitting the subject, and as I decline all controversy, I deprecate criticism, and trust they will be taken for what they are intrinsically worth, and no more.

(Signed)

W. H. BURRELL, M.D.

*Staff Surgeon, 1st Class.*

*London, May 29, 1850.*



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